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COSMICAL PHYSICS[1]

To one who has spent many years over the solution of a problem which is somewhat isolated from the more general questions of his subject, it is a satisfaction to have this opportunity for presenting the problem as a whole instead of in the piecemeal fashion which is necessary when there are many separate features to be worked out. In doing so, I shall try to avoid the more technical details of my subject as well as the temptation to enter into closely reasoned arguments, confining myself mainly to the results which have been obtained and to the conclusions which may be drawn from them.

In setting forth the present status of the problem, another side of it gives one a sense of pleasure. When a comparison between the work of the lunar theorist and that of the observer has to be made, it is necessary to take into consideration the facts and results obtained by astronomers for purposes not directly connected with the moon: the motions of the earth and planets, the position of the observer, the accuracy of star catalogues, the errors of the instruments used for the measurement of the places of celestial objects, the personality of the observers—all these have to be considered; in fact, almost every one of the departments of the astronomy of position must be drawn upon to furnish necessary data. The time has now arrived when it may perhaps be possible to repay in some measure the debt thus contracted by furnishing to the astronomer, and perhaps

[1] Address of the Vice-president of Section A, British Association for the Advancement of Science, Australasian meeting, 1914.

also to the student of geodesy and, if I may coin a word, of selenodesy, some results which can be deduced more accurately from a study of the moon's motion than in any other way. A long-continued exploration with few companions which ultimately leads to territories where other workers have already blazed paths gives the impression of having emerged from the thick jungle into open country. The explorer can once more join forces with his brother astronomers. He can judge his own results more justly and have them judged by others. If, then, an excuse be needed for overstepping the limits which seem, by silent consent, to have been imposed on those who devote themselves to lunar problems, it consists in a desire to show that these limits are not necessary and that a study of the motion of the moon can be of value and can contribute its share to the common funds of astronomy.

The history of the motion of the moon has been for more than two centuries a struggle between the theorists and the observers. Ever since the publication of the "Principia" and the enunciation of the law of gravitation by Isaac Newton, a constant effort has been maintained to prove that the moon, like the other bodies of the solar system, obeyed this law to its farthest consequences. While the theory was being advanced, the observers were continually improving their instruments and their methods of observing, with the additional advantage that their efforts had a cumulative effect: the longer the time covered by their observations, the more exact was the knowledge obtained. The theorist lacked the latter advantage: if he started anew he could only use the better instruments for analysis provided by the mathematician. He was always trying to forge a plate of armor which the observer with a gun whose power was increasing with the time could not penetrate. In the struggle the victory rarely failed to rest with the observer. Within the last decade we theorists have made another attempt to forge a new plate out of the old materials; whether we have substantially gained the victory must rest partly on the evidence I have to place before you to-day and partly on what the observer can produce in the near future.

There are three well-defined periods in the history of the subject as far as a complete development of the moon's motion is concerned. From the publication of the "Principia" in 1687, when Newton laid down the broad outlines, until the middle of the eighteenth century, but little progress was made. It seems to have required over half a century for analysis by symbols to advance sufficiently far for extensive applications to the problems of celestial mechanics. Clairaut and d'Alembert both succeeded in rescuing the problem from the geometrical form into which Newton had cast it and in reducing it to analysis by the methods of the calculus. They were followed by Leonard Euler, who in my opinion is the greatest of all the successors of Isaac Newton as a lunar theorist. He initiated practically every method which has been used since his time, and his criticisms show that he had a good insight into their relative advantages. A long roll of names follows in this period. It was closed by the publication of the theories of Delaunay and Hansen and the tables of the latter, shortly after the middle of the nineteenth century. From then to the end of the century the published memoirs deal with special parts of the theory or with its more general aspects, but no complete development appeared which could supersede the results of Hansen.

My own theory, which was completed a few years ago, is rather the fulfilment to the utmost of the ideas of others than a

new mode of finding the moon's motion. Its object was severely practical—to find in the most accurate way and by the shortest path the complete effect of the law of gravitation applied to the moon. It is a development of Hill's classic memoir of 1877. Hill in his turn was indebted to some extent to Euler. His indebtedness would have been greater had he been aware of a little-known paper of the latter, "Sur la Variation de la Lune," in which the orbit, now called the variation orbit, is obtained, and its advantages set forth in the words: "Quelque chimérique cette question j'ose assurer que, si l'on réussissoit à en trouver une solution parfaite on ne trouveroit presque plus de difficulté pour déterminer le vrai mouvement de la Lune réelle. Cette question est donc de la dernière importance et il sera toujours bon d'en approfondir toutes les difficultés, avant qu'on en puisse espérer une solution complète."

In the final results of my work the development aims to include the gravitational action of every particle of matter which can have a sensible effect on the moon's motion, so that any differences which appear between theory and observation may not be set down to want of accuracy in the completeness with which the theory is carried out. Every known force capable of calculation is included.

So much for the theory. Gravitation, however, is only a law of force: we need the initial position, speed and direction of motion. To get this with sufficient accuracy no single set of observations will serve; the new theory must be compared with as great a number of these as possible. To do this directly from the theory is far too long a task and, moreover, it is not necessary. In the past every observation has been compared with the place shown in the "Nautical Almanac" and the small differences between them have been recorded from day to day. By taking many of these differences and reducing them so as to correspond with differences at one date, the position of the moon at that date can be found with far greater accuracy than could be obtained through any one observation. At the Greenwich Observatory the moon has been observed and recorded regularly since 1750. With some 120 observations a year, there are about 20,000 available for comparison, quite apart from shorter series at other observatories. Unfortunately these observations are compared with incorrect theories, and, in the early days, the observers were not able to find out, with the accuracy required to-day, the errors of their instruments or the places of the stars with which the moon was compared. But we have means of correcting the observations, so that they can be freed from many of the errors present in the results which were published at the time the observations were made. We can also correct the older theories. They can be compared with the new theory and the differences calculated: these differences need not even be applied to the separate observations, but only to the observations combined into properly chosen groups. Thus the labor involved in making use of the earlier observations is much less than might appear at first sight.

For the past eighteen months I have been engaged in this work of finding the differences between the old theories and my own, as well as in correcting those observations which were made at times before the resources of the astronomer had reached their present stage of perfection. I have not dealt with the observations from the start: other workers, notably Airy in the last century and Cowell in this, have done the greater part of the labor. My share was mainly to carry theirs a stage further by adopting the latest theory and the best modern practise for the reduction of the

observations. In this way a much closer agreement between theory and observation has been obtained, and the initial position and velocity of the moon at a given date are now known with an accuracy comparable with that of the theory. I shall shortly return to this problem and exhibit this degree of accuracy by means of some diagrams which will be thrown on the screen.

I have spoken of the determination of these initial values as if it constituted a problem separate from the theory. Theoretically it is so, but practically the two must go together. The increase in accuracy of the theory has gone on successively with increase in accuracy of the determination of these constants. We do not find, with a new theory, the new constants from the start, but corrections to the previously adopted values of these constants. In fact, all the problems of which I am talking are so much interrelated that it is only justifiable to separate them for the purposes of exposition.

Let us suppose that the theory and these constants have been found in numerical form, so that the position of the moon is shown by means of expressions which contain nothing unknown but the time. To find the moon's place at any date we have then only to insert that date and to perform the necessary numerical calculations. This is not done directly, on account of the labor involved. What are known as "Tables of the Moon's Motion" are formed. These tables constitute an intermediate step between the theory and the positions of the moon which are printed in the "Nautical Almanac." Their sole use and necessity is the abbreviation of the work of calculation required to predict the moon's place from the theoretical values which have been found. For this reason, the problem of producing efficient tables is not properly scientific: it is mainly economic. Nevertheless, I have found it as interesting and absorbing as any problem which involves masses of calculation is to those who are naturally fond of dealing with arithmetical work. My chief assistant, Mr. H. B. Hedrick, has employed his valuable experience in helping me to devise new ways of arranging the tables and making them simple for use.

A table is mainly a device by which calculations which are continually recurring are performed once for all time, so that those who need to make such calculations can read off the results from the table. In the case of the moon, the tables go in pairs. Each term in the moon's motion depends on an angle, and this angle depends on the date. One table gives the value of the angle at any date (a very little calculation enables the computer to find this), and the second table gives the value of the term for that angle. As the same angles are continually recurring, the second table will serve for all time.

We can, however, do better than construct one table for each term. The same angle can be made to serve for several terms and consequently one table may be constructed so as to include all of them. In other words, instead of looking out five numbers for five separate terms, the computer looks out one number which gives him the sum of the five terms. The more terms we can put into a single table the less work for the astronomer who wants the place of the moon, and therefore the more efficient the tables. A still better device is a single table which depends on two angles, known as a double-entry table; many more terms can usually be included in this than in a single-entry table. The double interpolation on each such table is avoided by having one angle the same for many double-entry tables and interpolating

for that angle on the sum of the numbers extracted from the tables.

The problem of fitting the terms into the smallest number of tables is a problem in combinations—something like a mixture of a game at chess and a picture-puzzle, but unlike the latter in the fact that the intention is to produce ease and simplicity instead of difficulty. This work of arrangement is now completed and, in fact, about five sixths of the calculations necessary to form the tables are done; over one third of the copy is ready for the printer, but, owing to the large mass of the matter, it will take from two to three years to put it through the press. The cost of performing the calculations and printing the work has been met from a fund specially set aside for the purpose by Yale University.

A few statistics will perhaps give an idea of our work. Hansen has 300 terms in his three coordinates, and these are so grouped that about a hundred tables are used in finding a complete place of the moon. We have included over 1,000 terms in about 120 tables, so that there are on the average about eight terms per table. In one of our tables we have been able to include no less than forty terms. Each table is made as extensive as possible in order that the interpolations—the bane of all such calculations—shall be easy. The great majority of them involve multiplications by numbers less than 100. There are less than ten tables which will involve multiplications by numbers between 100 and 1,000 and none greater than the latter number. The computer who is set to work to find the longitude, latitude and parallax of the moon will not need a table of logarithms from the beginning to the end of his work. The reason for this is that all multiplications by three figures or less can be done by Crelle's well-known tables or by a computing machine. But Mr. Hedrick has devised a table for interpolation to three places which is more rapid and easy than either of these aids. It is, of course, of use generally for all such calculations, and arrangements are now being made for the preparation and publication of his tables. The actual work of finding the place of the moon from the new lunar tables will, I believe, not take more time—perhaps less—than from Hansen's tables, as soon as the computer has made himself familiar with them. Fortunately for him, it is not necessary to understand the details of their construction: he need only know the rules for using them.

I am now going to show by means of some diagrams the deviations of the moon from its theoretical orbit, in which, of course, errors of observation are included. The first two slides exhibit the average deviation of the moon from its computed place for the past century and a half in longitude.[2] The averages are taken over periods of 414 days and each point of the continuous line shows one such average. The dots are the results obtained by Newcomb from occultations; the averages for the first century are taken over periods of several years, and in the last sixty years over every year. In both cases the same theory and the same values of the constants have been used. Only one empirical term has been taken out—the long-period fluctuation found by Newcomb having a period of 270 years and a coefficient of 13″. I shall show the deviations with this term included, in a moment.

The first point to which attention should be drawn is the agreement of the results deduced from the Greenwich meridian observations and those deduced from occultations gathered from observatories all over the world. There can be no doubt that the fluctuations are real and not due to errors

[2] *Monthly Notices R.A.S.*, Vol. 73, plate 22.

of observation. A considerable difference appears about 1820, for which I have not been able to account, but I have reasons for thinking that the difference is mainly due to errors in the occultations rather than in the meridian values. In the last sixty years the differences become comparatively small, and the character of the deviation of the moon from its theoretical orbit is well marked. This deviation is obviously of a periodic character, but attempts to analyze it into one or two periodic terms have not met with success; the number of terms required for the purpose is too great to allow one to feel that they have a real existence, and that they would combine to represent the motion in the future. The straight line character of the deviations is a rather marked peculiarity of the curves.

The actual deviations on a smaller scale are shown in the next slide; the great empirical term has here been restored and is shown by a broken line. The continuous line represents the Greenwich meridian observations; the dots are Newcomb's results for the occultations before 1750, the date at which the meridian observations begin. With a very slight amount of smoothing, especially since 1850, this diagram may be considered to show the actual deviations of the moon from its theoretical orbit.

The next slide shows the average values of the eccentricity and of the position of the perigee.[3] The deviations are those from the values which I have obtained. It is obvious at once that there is little or nothing systematic about them; they may be put down almost entirely to errors of observation. The diminishing magnitude of the deviations as time goes on is good evidence for this; the accuracy of the observations has steadily increased. The coefficient of the term on which the eccentricity depends is found with a probable error of $0''.02$, and the portion from 1750 to 1850 gives a value for it which agrees with that deduced from the portion 1850 to 1901 within $0''.01$. The eccentricity is the constant which is now known with the highest degree of accuracy of any of those in the moon's motion. For the perigee there was a difference from the theoretical motion which would have caused the horizontal average in the curve to be tilted up one end over $2''$ above that at the other end. I have taken this out, ascribing it to a wrong value for the earth's ellipticity; the point will be again referred to later. The actual value obtained from the observations themselves has been used in the diagram, so that the deviations shown are deviations from the observed value.

The next slide shows the deviations of the mean inclination and the motion of the node, as well as of the mean latitude from the values deduced from the observations.[4] In these cases the observations only run from 1847 to 1901. It did not seem worth while to extend them back to 1750 for it is evident that the errors are mainly accidental, and the mean results agreed so closely with those obtained by Newcomb from occultations that little would have been gained by the use of the much less accurate observations made before 1847. The theoretical motion of the node differs from its observed value by a quantity which would have tilted up one end of the zero line about $0''.5$ above the other; the hypothesis adopted in the case of the perigee will account for the difference.

The mean latitude curve is interesting. It should represent the mean deviations of the moon's center from the ecliptic; but

3 Tables II., III. of a paper on "The Perigee and Eccentricity of the Moon," *Monthly Notices R.A.S.*, March, 1914.

4 "The Mean Latitudes of the Sun and Moon," *Monthly Notices R.A.S.*, January, 1914; "The Determination of the Constants of the Node, the Inclination, the Earth's Ellipticity, and the Obliquity of the Ecliptic," *ib.*, June, 1914.

it actually represents the deviations from a plane 0''.5 below the ecliptic. A similar deviation was found by Newcomb. Certain periodic terms have also been taken out. The explanation of these terms will be referred to directly.

The net result of this work is a determination of the constants of eccentricity, inclination, and of the positions of the perigee and node with practical certainty. The motions of the perigee and node here agree with their theoretical values when the new value of the earth's ellipticity is used. The only outstanding parts requiring explanation are the deviations in the mean longitude. If inquiry is made as to the degree of accuracy which the usual statement of the gravitation law involves, it may be said that the index which the inverse square law contains does not differ from 2 by a fraction greater than 1/400,000,000. This is deduced from the agreement between the observed and theoretical motions of the perigee when we attribute the mean of the differences found for this motion and for that of the node to a defective value of the ellipticity of the earth.

I have mentioned the mean deviation of the latitude of the moon from the ecliptic. There are also periodic terms with the mean longitude as argument occurring both in the latitude and the longitude. My explanation of these was anticipated by Professor Bakhuysen by a few weeks. The term in longitude had been found from two series of Greenwich observations, one of 28 and the other of 21 years, by van Steenwijk, and Professor Bakhuysen, putting this with the deviations of the mean latitude found by Hansen and himself, attributed them to systematic irregularities of the moon's limbs.

What I have done is to find (1) the deviation of the mean latitude for 64 years, (2) a periodic term in latitude from observations covering 55 years, and (3) a periodic term in longitude from observations covering 150 years, the period being that of the mean longitude. Further, if to these be added Newcomb's deviations of the mean latitude derived (*a*) from immersions and (*b*) from emersions, we have a series of five separate determinations—separate because the occultations are derived from parts of the limb not wholly the same as those used in meridian observations. Now all these give a consistent shape to the moon's limb referred to its center of mass. This shape agrees qualitatively with that which may be deduced from Franz's figure.

I throw on the screen two diagrammatic representations[5] of these irregularities obtained by Dr. F. Hayn from a long series of actual measures of the heights and depths of the lunar formations. The next slide shows the systematic character more clearly. It is from a paper by Franz.[6] It does not show the character of the heights and depths at the limb, but we may judge of these from the general character of the high and low areas of the portions which have been measured and which extend near to the limbs. I think there can be little doubt that this explanation of these small terms is correct, and if so it supplies a satisfactory cause for a number of puzzling inequalities.

The most interesting feature of this result is the general shape of the moon's limb relative to the center of mass and its relation to the principle of isostasy. Here we see with some definiteness that the edge of the southern limb in general is further from the moon's center of mass than the northern. Hence we must conclude that the density at least of the crust of the former is less than that of the latter, in accordance with

[5] *Abh. der Math.-Phys. Kl. der Kön. Sächs. Ges. der Wiss.*, Vols. XXIX., XXX.

[6] *Königsberger Astr. Beob.*, Abth. 38.

the principle mentioned. The analogy to the figure of the earth with its marked land and sea hemispheres is perhaps worth pointing out, but the higher ground in the moon is mainly on the south of its equator, while that on the earth is north. Unfortunately we know nothing about the other face of the moon. Nevertheless it seems worth while to direct the attention of geologists to facts which may ultimately have some cosmogonic applications. The astronomical difficulties are immediate: different corrections for meridian observations in latitude, in longitude, on Mösting A, for occultations and for the photographic method, will be required.

I next turn to a question, the chief interest of which is geodetic rather than astronomical. I have mentioned that a certain value of the earth's ellipticity will make the observed motions of the perigee and node agree with their theoretical values. This value is $1/293.7 \pm .3$. Now Helmert's value obtained from gravity determinations is 1/298.3. The conference of "Nautical Almanac" Directors in 1911 adopted 1/297. There is thus a considerable discrepancy. Other evidence, however, can be brought forward. Not long ago a series of simultaneous observations at the Cape and Greenwich Observatories was made in order to obtain a new value of the moon's parallax. After five years' work a hundred simultaneous pairs were obtained, the discussion of which give evidence of their excellence. Mr. Crommelin, of the Greenwich Observatory, who undertook this discussion, determined the ellipticity of the earth by a comparison between the theoretical and observed values of the parallax. He found an ellipticity $1/294.4 \pm 1.5$ closely agreeing with that which I have obtained. Finally, Col. Clarke's value obtained from geodetic measures was 1/293.5. We have thus three quite different determinations ranging round 1/294 to set against a fourth determination of 1/298. The term in the latitude of the moon which has often been used for this purpose is of little value on account of the coefficient being also dependent on the value of the obliquity of the ecliptic; such evidence as it presents is rather in favor of the larger value. I omit Hill's value, obtained from gravity determinations, because it is obviously too large.

Here, then, is a definite issue. To satisfy the observations of the moon in at least three different parts, a value near 1/294 must be used; while the value most carefully found from gravity determinations is 1/298. As far as astronomy is concerned, the moon is the only body for which a correct value of this constant is important, and it would seem inadvisable to use a value which will cause a disagreement between theory and observation in at least three different ways. It is a question whether the conference value should not be changed with the advent of the new lunar tables.

In looking forward to future determinations of this constant, it seems to be quite possible that direct observations of the moon's parallax are likely to furnish at least as accurate a value of the earth's shape as any other method. This can be done, I believe, much better by the Harvard photographic method than by meridian observations. Two identical instruments are advisable for the best results, one placed in the northern and the other in the southern hemisphere from 60° to 90° apart in latitude and as nearly as possible on the same meridian. On nights which are fine at both stations, from fifteen to twenty pairs of plates could be obtained. In a few months it is probable that some 400 pairs might be obtained. These should furnish a value for the parallax with a probable error of about $0''.02$ and a value for the ellipticity within half a unit of the denom-

inator 294. It would be still more interesting if the two instruments could be set up on meridians in different parts of the earth. The Cape and a northern observatory, Upsala for example, would furnish one arc; Harvard and Arequipa or Santiago another. If it were possible to connect by triangulation Australia with the Asiatic continent, a third could be obtained near the meridian of Brisbane. Or, accepting the observed parallax and the earth's ellipticity, we could find by observation the lengths of long arcs on the earth's surface with high accuracy.

In any case, I believe that the time must shortly come when the photographic method of finding the moon's place should be taken up more extensively, whether it be used for the determination of the moon's parallax and the earth's ellipticity or not. The Greenwich meridian observations have been and continue to be a wonderful storehouse for long series of observations of the positions of the sun, moon, planets and stars. In the United States, Harvard Observatory has adopted the plan of securing continuous photographic records of the sky with particular reference to photometric work. Under Professor Pickering it will also continue the photographic record of the moon's position as long as arrangements can be made to measure the plates and compute the moon's position from them.

In spite of the fact that Harvard Observatory has undertaken to continue for the present the work of photographing the moon's position, I believe that this method should find a permanent home in a national observatory. It has already shown itself capable of producing the accuracy which the best modern observations of Greenwich can furnish, and no higher praise need be given. If this home could be found in the southern hemisphere, and more particularly in Australia, other advantages would accrue.

But we should look for more than this. In an observatory whose first duty might be the securing of the best daily records of the sky, the positions of the sun, stars, planets, a couple of plates of the moon on every night when she is visible would be a small matter. What is needed is an organization so constructed as to be out of the reach of changing governmental policy with a permanent appropriation and a staff of the highest character removed from all political influences. It could render immense service to astronomers, not only in the Empire but all over the world. The pride which every Englishman feels who has to work with the records of the past furnished by Greenwich would in course of time arise from the work of a similar establishment elsewhere. Those of us who live in a community which, reckoning by the age of nations, is new, know that, in order to achieve objects which are not material, sacrifices must be made; but we also know that such sacrifices are beneficial, not only in themselves, but as exerting an indirect influence in promoting the cause of higher education and of scientific progress in every direction. In saying this I am not advocating the cause of the few, but of the majority; the least practical investigations of yesterday are continually becoming of the greatest practical value to-day.

No address before this section is complete without some speculation and a glance towards the future. I shall indulge in both to some small extent before closing. I have shown you what the outstanding residuals in the moon's motion are: they consist mainly of long-period fluctuations in the mean longitude. I have not mentioned the secular changes because the evidence for them does not rest on modern observations but on ancient eclipses, and these are matters too debatable to discuss in the limited time allotted to me for this address. It may be said, however, that the only

secular motion which is capable of being determined from the modern observations and is not affected by the discussion of ancient eclipses—namely, the secular motion of the perigee—agrees with its theoretical value well within the probable error. With this remark I pass to the empirical terms.

These unexplained differences between theory and observation may be separated into two parts. First, Newcomb's term of period between 250 and 300 years and coefficient 13″, and, second, the fluctuations which appear to have an approximate period of 60 to 70 years. The former appears to be more important than the latter, but from the investigator's point of view it is less so. The force depends on the degree of inclination of the curve to the zero line or on the curvature, according to the hypothesis made. In either case the shorter period term is much more striking, and, as I have pointed out on several occasions, it is much more likely to lead to the sources of these terms than the longer period. It is also, at least for the last sixty years, much better determined from observation, and is not likely to be confounded with unknown secular changes.

Various hypotheses have been advanced within the last few years to account for these terms. Some of them postulate matter not directly observed or matter with unknown constants; others, deviations of the Newtonian law from its exact expression; still others, non-gravitational forces. M. St. Blancat[7] examines a variety of cases of intramercurial planets and arrives at the conclusion that such matter, if it exists, must have a mass comparable with that of Mercury. Some time ago I examined the same hypothesis and arrived at similar results. The smallest planet with density four times that of water, which would produce the long inequality, must have a disc of nearly 2″ in its transit across the sun and a still larger planet would be necessary to produce the shorter period terms. But observational attempts, particularly those made by Perrine and Campbell, have always failed to detect any such planet, and Professor Campbell is of the opinion that a body with so large a disc could hardly have been overlooked. If we fall back on a swarm instead of a single body, we replace one difficulty by two. The light from such a swarm would be greater than that from a single body, and would therefore make detection more likely. If the swarm were more diffused we encounter the difficulty that it would not be held together by its own attraction, and would therefore soon scatter into a ring; such a ring can not give periodic changes of the kind required.

The shading of gravitation by interposing matter, *e. g.*, at the time of eclipses, has been examined by Bottlinger.[8] For one reason alone, I believe this is very doubtful. It is difficult to see how new periodicities can be produced; the periods should be combinations of those already present in the moon's motion. The sixty to seventy years' fluctuation stands out in this respect because its period is not anywhere near any period present in the moon's motion or any probable combination of the moon's periods. Indeed Dr. Bottlinger's curve shows this: there is no trace of the fluctuation.

Some four years ago I examined[9] a number of hypotheses. The motions of the magnetic field of the earth and of postulated fields on the moon had to be rejected, mainly because they caused impossible increases in the mean motion of the perigee. An equatorial ellipticity of the sun's mass,

[7] *Annales de la Faculté des Sciences de Toulouse*, 1907.

[8] *Diss., Freiburg* i. Br., 1912.

[9] *Amer. Jour. Sc.*, Vol. 29.

combined with a rotation period very nearly one month in length, appeared to be the best of these hypotheses. The obvious objections to it are, first, that such an ellipticity, small as it can be (about 1/20,000), is difficult to understand on physical grounds, and, second, that the rotation period of the nucleus which might be supposed to possess this elliptic shape in the sun's equator is a quantity which is so doubtful that it furnishes no help from observation, although the observed periods are well within the required limits. Dr. Hale's discovery of the magnetic field of the sun is of interest in this connection. Such a field, of non-uniform strength, and rotating with the sun, is mathematically exactly equivalent to an equatorial ellipticity of the sun's mass, so that the hypothesis might stand from the mathematical point of view, the expression of the symbols in words being alone different.

The last-published hypothesis is that of Professor Turner,[10] who assumes that the Leonids have finite mass and that a big swarm of them periodically disturbs the moon as the orbits of the earth and the swarm intersect. I had examined this myself last summer, but rejected it because, although it explained the straight line appearance of the curve of fluctuations, one of the most important of the changes of direction in this curve was not accounted for. We have the further difficulty that continual encounters with the earth will spread the swarm along its orbit, so that the swarm with this idea should be a late arrival and its periodic effect on the moon's motion of diminishing amplitude; with respect to the latter, the observed amplitude seems rather to have increased.

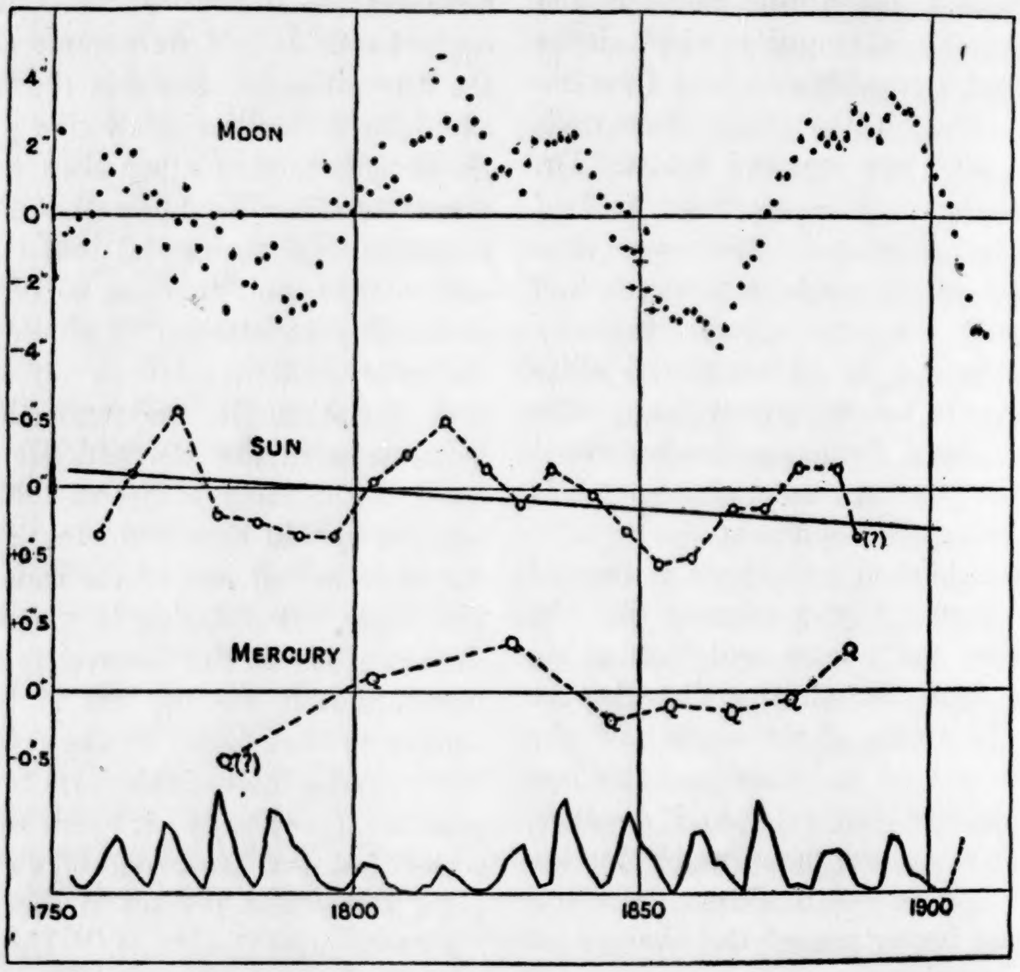
The main objection to all these ideas consists in the fact that they stand alone: there is as yet little or no collateral evidence from other sources. The difficulty, in fact, is not that of finding a hypothesis to fit the facts, but of selecting one out of many. The last hypothesis which I shall mention is one which is less definite than the others, but which does appear to have some other evidence in its favor.

[10] *Monthly Notices*, December, 1913.

The magnetic forces, mentioned above, were changes in the *directions* of assumed magnetic fields. If we assume changes in the intensities of the fields themselves, we avoid the difficulties of altering portions of the moon's motion other than that of the mean motion. We know that the earth's magnetic field varies and that the sun has such a field, and there is no inherent improbability in attributing similar fields to the moon and the planets. If we assume that variations in the strength of these fields arise in the sun and are communicated to the other bodies of the solar system, we should expect fluctuations having the same period and of the same or opposite phase but differing in magnitude. It therefore becomes of interest to search for fluctuations in the motions of the planets similar to that found in the moon's orbit. The material in available form for this purpose is rather scanty; it needs to be a long series of observations reduced on a uniform plan. The best I know is in Newcomb's "Astronomical Constants." He gives there the material for the earth arranged in groups of a few years at a time. The results for Mercury, given for another purpose, can also be extracted from the same place. For Venus and Mars, Newcomb unfortunately only printed the normal equations from which he deduces the constants of the orbit.

On the screen is shown a slide which exhibits the results for the earth and Mercury compared with those for the moon. In the uppermost curve are reproduced the minor fluctuations of the moon shown

earlier; the second curve contains those of the earth's longitude; the third, those of Mercury's longitude. [By accident the mean motion correction has been left in the earth curve; the zero line is therefore inclined instead of being horizontal.] It will be noticed that the scales are different and that the earth curve is reversed. In spite of the fact that the probable errors of the results in the second and third curves are not much less than their divergencies from a straight line, I think that the correlation exhibited is of some significance. If it is, we have here a force whose period, if period in the strict sense it has, is the same as that of the effect: the latter is not then a resonance from combination with another period. We must therefore look for some kind of a surge spreading through the solar system and affecting planets and satellites the same way but to different degrees.



The lowest curve is an old friend, that of Wolf's sunspot frequency, put there, not for that reason, but because the known connection for the last sixty years between sunspot frequency and prevalence of magnetic disturbance enables us with fair probability to extend the latter back to 1750. With some change of phase the periods of high and low maxima correspond nearly with the fluctuations above. The eleven-year oscillation is naturally eliminated from the group results for the earth and Mercury. One might expect it to be present in the lunar curve, but owing to its shorter period we should probably not obtain a coefficient of over half a second. Notwithstanding this fact, it is a valid objection to the hypothesis that there is no evi-

dence of it in the moon's motion. Reasons may exist for this: but until the mechanism of the action can be made more definite it is hardly worth while to belabor the point.

The hypothesis presents many difficulties. Even if one is disposed to admit provisionally a correlation between the four curves —and this is open to considerable doubt— it is difficult to understand how, under the electron theory of magnetic storms, the motions of moon and planets can be sensibly affected. I am perhaps catching at straws in attempting to relate two such different phenomena with one another, but when we are in the presence of anomalies which show points of resemblance and which lack the property of analysis into strict periodic sequences some latitude may be permissible.

In conclusion, what, it may well be asked, is the future of the lunar theory now that the gravitational effects appear to have been considered in such detail that further numerical work in the theory is not likely to advance our knowledge very materially? What good purpose is to be served by continuous observation of the moon and comparison with the theory? I believe that the answer lies mainly in the investigation of the fluctuations already mentioned. I have not referred to other periodic terms which have been found because the observational evidence for their real existence rests on foundations much less secure. These need to be examined more carefully, and this examination must, I think, depend mainly on future observations rather than on the records of the past. Only by the greatest care in making the observations and in eliminating systematic and other errors from them can these matters be fully elucidated. If this can be achieved and if the new theory and tables serve, as they should, to eliminate all the known effects of gravitation, we shall be in a position to investigate with some confidence the other forces which seem to be at work in the solar system and at which we can now only guess. Assistance should be afforded by observations of the sun and planets, but the moon is nearest to us and is, chiefly on that account, the best instrument for their detection. Doubtless other investigations will arise in the future. But the solution of the known problems is still to be sought, and the laying of the coping stone on the edifice reared through the last two centuries can not be a simple matter. Even our abler successors will hardly exclaim, with Hotspur,

> By heaven, methinks, it were an easy leap
> To pluck bright honor from the pale-faced moon.

They, like us and our predecessors, must go through long and careful investigations to find out the new truths before they have solved our difficulties, and in their turn they will discover new problems to solve for those who follow them:

> "For the fortune of us, that are the moon's men, doth ebb and flow like the sea, being governed, as the sea is, by the moon."

E. W. BROWN

BOTANY IN THE AGRICULTURAL COLLEGE

FIVE years ago, there was, I believe, no college in the United States which required that plant physiology be studied by any student of agriculture. There were a very few colleges in which it was possible for students of agriculture to take as much as one year's work in this subject, but the number of such places was exceedingly limited and remains so. The college of agriculture of the University of the Philippines was founded at that time; and having a free hand in planning its course of study, I provided that every student not only could but must take one full year of plant physiology, and that students taking the course regularly must have this year of physiology before being admitted to the study of agriculture itself.

There were several reasons for taking this rather radical step. Decidedly the strongest

of these was the obvious fact that the raising of crops is essentially nothing more or less than applied botany. The botany which is useful in plant industry is not a study of the names of cultivated plants and weeds, and not primarily the cataloguing of plant products and plant diseases, but is the phase of botany which treats of the responses of plants to the conditions under which they grow. This is plant physiology. It is a phase of botany which can not be taught to students who have not some previous general knowledge of plants. It is here taught to students who have had one year of general botany, but who have not yet any chemistry. Some have had physics and some have not. It would be impossible to give to our students the sort of a quasi-cultural subject which is usually presented where plant physiology is taught at all in the United States. But a considerable part of the American course which could not be given to our students would likewise be useless to them. Our course in plant physiology is planned specifically to give students such an understanding of the behavior of plants as should serve as a guide in the treatment of crops.

This course was put in operation here without any advertising; while I felt perfectly sure that the proposition that the best scientific foundation for plant industry is a knowledge of plant physiology, is a sound one, I felt also that the general respect for a widely adopted system would be so strong that a radical experiment of this kind would be sure to receive very little favorable attention until it had been well tested in practise. Five years' experience should be enough to put a plan, which may have looked like an experiment at its first trial, on a different basis. During these five years, the same plan in greater dilution has been applied in some places in the United States. The example of Wisconsin in requiring half a year of plant physiology of students in some agricultural courses may have more weight in commending the subject than does our local experience with a full year. The desirable thing is that the value of the subject be recognized, and it is to be hoped that Wisconsin's example will be followed by other institutions as fast as is in their power.

It has already been stated that our course in physiology is fitted directly to the practical purpose it is to serve. For this purpose, growth receives particularly careful study. The student is drilled in growth measurements until he regards them as a matter of course, rather than as experiments. The number of such measurements required of each student is approximately 3,000. Aside from giving a thorough first-hand idea of the growth of a variety of plants and of different parts of plants under various treatment, this extensive drill has the practical result that the student acquires speed and accuracy in such work, such that if he is afterward called upon to determine how fast the plants in a corn field or coconut or coffee plantation are growing, he goes at it with skill and confidence. I have asked a number of graduates of American agricultural colleges how fast corn should grow at different ages; not one could give anything better than a relative and altogether indefinite answer. Not one of them knew anything about it from personal observation of a single plant. Not one had, so far as he remembered, even been given a figure on the subject; let alone being called upon to fix it in his mind by finding out for himself. Not one had any standard by which he could state that a plant or a field of corn was growing as it should, or doing better or worse. It seems to me that the graduate of an agricultural college should know a good deal more about the behavior of corn than any of these graduates do know. It would amaze our students if they were made to realize that a student could graduate from a famous agricultural college in a state where corn is a leading crop, without ever following through the growth of a single corn plant. Corn has received a careful study of just this kind in the United States, and is, so far as I know, the only American crop which has received a really careful study of this kind.

Next after growth in the attention it receives is transpiration. Next in order is direct study of the mineral food of plants, and

nitrogen. Transpiration receives more careful study because it is of importance in a number of aspects. Without water in which plants can absorb it, mineral food is as useless in the soil as it is in a warehouse. Mineral food is studied in water culture, in pot culture, and in beds on the farm. In water culture, the work is made as exact as it possibly can be, using chemically pure salts, distilled water, and the most insoluble containers. In the field the work is made as practical as it can be, using ordinary garden crops on ordinary farm soil, with the fertilizers which are regarded as generally available. As a matter of interest, manure, ashes and commercial fertilizers used in this experiment are analyzed on the grounds, and the results of the analysis given to the students. But the experiment is intended and understood to show the students what results they can obtain at home by methods of procedure which are practicable there. Work on such a scale as these fertilizer experiments must be done by groups instead of by individual students, else the course will demand more time than can be found in a single year.

Other phases of plant physiology receive less attention. There are a reasonable number of experiments on photosynthesis. But, important as it is, this phase of plant activity is relatively not subject to direct human control; a thorough familiarity with it is accordingly of much less practical utility. The study of respiration is still briefer. Such subjects as geotropism, and the others sometimes grouped under the head of "Irritability," are treated briefly in lectures, and passed over with an easy experiment or two, not requiring more than a day each in the laboratory.

Practically in the place of this, the student in the American college of agriculture is taught chemistry. Chemistry is of course a necessary part of agricultural education. Our students study it for two years. But the plant physiology and not the chemistry is the basis on which their agriculture rests.

The difference in the scientific foundation makes the instruction in plant industry itself different. Our courses in agronomy are full of plant physiology. In these cases, the special plant physiology of the particular crops has thorough study. Thus, the students of the coconut measure the growth of leaf, root, flowering branch and fruit. The growth of the leaf is the easiest index to the general activity of the tree, and accordingly receives most attention. This work has now been carried on so long, and such a mass of data has been accumulated that it is possible to establish a figure which represents satisfactory activity, and to determine approximately how much this varies with the change in weather from day to day. With this information, the student can go into a coconut plantation and determine with a high measure of probability the average production from the grove two years and a half hence; and he can do this after 24 hours' observation. The estimate he makes is a very much more reliable one than can be made from a three months' study of the present rate of production. We expect to establish standards of this kind for all of our principal crops. But to do so, and get figures which can be relied upon, is no small task. On the coconut we have more than 100,000 single measurements of rate of growth. Standards of this kind are certainly worth having. I do not think it admits of question that the ability to use, and if need be to make them, is a valuable part of a student's education.

The student also measures the absorption of water by the roots of the coconut and its transpiration from the leaves, and the absorption of mineral food by the roots. He learns how much water the plant needs, and how it responds to differences in the water supply. When he gets done, he knows enough about the physiology of the coconut to realize that soil analysis, or even the decidedly more useful analysis of the parts of the plant, will not, by itself, give him any idea of whether or not it is worth while to apply fertilizers. He knows that if his trees are getting less mineral food than they should, it may be impossible to remedy the deficiency by buying fertilizers, and that the difficulty frequently can be remedied, and remedied more cheaply, by the use of water. In short, he understands the

behavior and the wants of the plants he is growing, and can accordingly treat them with a degree of intelligence which can not be hoped for from those who have not become familiar with the practical phases of plant physiology.

The object of agricultural education is to produce farmers who will do their work intelligently. Speaking for plant industry alone, the most essential part of such a training is the acquisition on the student's part of the kind of understanding of plants, and particularly of the plants which he will raise, which he can get from the study of their physiology, and in no other way. The name of the study is of course of no importance. If it be chemistry of the kind represented by Adolf Mayer's "Agricultural Chemistry," or physics of the type of Wollny's "Agricultural Physics," the aim is reached. Both of these are plant physiology under other names which do not hurt them. But I do not believe that a student ever came out of an American college of agriculture, trained in physics or chemistry of this kind.

Although there has not been time for so much experience on this point, I believe that the advantage in our method of training goes well beyond the preparation for farming. By giving the student a more intelligent understanding of the behavior of his crops, we must give him a more intelligent interest in the problems of the farm. Up to this time, every one of our graduates is still a student or is engaged in agricultural work. Some are farming, some are employed by the Insular Bureau of Agriculture, and some are teaching agriculture. It is of course not to be expected that *all* of our graduates will always stick to the profession. But I am very confident that a larger proportion of them will do so than would if their training had been of the usual American kind. I had a chance two years ago to question a number of students about to graduate in agriculture at one of the foremost colleges in the United States. To the first questions, they all answered alike, that they study agriculture in college for the purpose of learning to farm scientifically; that the scientific basis of agriculture, as they had learned it, was chemistry; and that the chemistry they had been taught was something they would be unable to put into individual practise as farmers. As to whether, if the chance had been given, they could have made better use of plant physiology as a basis of agriculture, some thought they could, and others had not come sufficiently into touch with the subject to have an opinion. They all agreed that their education had failed to give them such an understanding of the problems of plant production, that they would be able, as individual farmers, to tackle its problems competently. In my opinion, the four years' instruction which had been given to them had failed essentially. Conscious inability to wrestle with problems is incompatible with an active interest in them.

The cities of the United States are growing at the expense of the country. It is universally agreed that the movement from country to city is a national calamity. The reason for this movement is not that the city offers greater prospect of material advance, for it does not do so. That life has been more comfortable and easier in the city has had something to do with this movement, but only a very minor part. Those who could live most comfortably on the farm, because of their means, have, on the whole, been most likely to move to the city. The essential cause of migration is that city life is interesting in a way which farm life is not. Neither bodily comfort, nor the certainty of such future success as will answer his needs, will keep the man who has the means to move to the city in a place where his mind is not interested. An agricultural education should of course qualify a man to farm with greater profit because of his education. But if it does not do more than this, if it does not give him a keen, intelligent interest in the problems he will encounter on the farm, it ought still to be counted a failure. To be really successful in their work, the agricultural colleges must send their graduates out so trained that the farm will present the fullest field for the activities of their minds. The successful agricultural college must train its students in such a way

that the city, and not the country, is too intolerably dull for a permanent residence. The American college of agriculture does not do this, and the main cause of its failure is that the kind of agricultural problems which are presented, discussed and worked with in its classes, are not the kind which it is practicable for a farmer to work with after he graduates. The graduate is not equipped to find employment for his intellect on the farm.

The theses in all this writing are:

First: the American college course in agriculture is basically wrong. Plant industry as a science must rest on an understanding of plants.

Second: the mistake of not giving this understanding results not merely in the waste of considerable time, and in making poorer farmers than might be produced, but results also in the failure of the college to check, as it should be expected to do, the movement, from the farm to the city, of the country's best blood.

E. B. COPELAND

SANITATION IN VERA CRUZ

THE Vera Cruz correspondent of the *Journal* of the American Medical Association writes that the hot season, which is also the rainy season, begins in Vera Cruz in May or June and lasts until the end of September, and as the season advances the tendency is for the death and morbidity rate for all diseases to increase, due to the heat itself, and the rapid increase in the amount of malaria; yet thanks to the effective work of our sanatoriums, this year is an exception, in that the civil death-rate for July is practically no greater than for June, in which month it was lower than the average. The civil death-rates per thousand of population, per annum, for the months of June and July for the past five years for the city of Vera Cruz are given below; the improvement for July of this year is too great to be accidental or due to anything but improved sanitation.

	June	July
1910	36.86	46.86
1911	38.29	46.86
1912	44.86	49.72
1913	36.86	41.15
1914	32.00	32.58

A comparative statement of the civil deaths from communicable diseases for June and July of this year is as follows:

	June	July
Typhoid fever	1	0
Malaria	8	2
Smallpox	4	1
Dysentery	12	4
Tuberculosis	19	26
Diarrhea and enteritis, under 2 years	19	14
Diarrhea and enteritis, 2 years and over	28	23

The increase of deaths from tuberculosis is not unusual during the hot weather; the smallpox epidemic is over and there are now no cases in the city; between May 18 and July 31, 66,432 persons were vaccinated; revaccinations are now being made when indicated but general vaccination ceased with the end of July. The principal gain is due to the fall in the death-rates for malarial and intestinal diseases and this improvement is directly due to our preventive measures.

The antimalarial measures which affect the civil population are three: the suppression of mosquito breeding, the use of the army laboratory in establishing the correct diagnosis, and the following up and treatment of all proved carriers of gametes in the blood. Mosquito-breeding has been largely suppressed by the extensive and intricate system of ditches in the environs of the city, totaling about 25 miles in length; miles of vacant lots and hundreds of acres of swamp at the bases of the gigantic sand-dunes behind the city have been drained by the Health Department, and it is now possible to sleep comfortably in almost all parts of the city without the use of mosquito-bars, something heretofore unknown at the height of the rainy season.

Malaria has been made a reportable disease by the Health Department and demonstration of the parasite in the blood is insisted on as far as possible. All houses where proved cases of malaria have occurred have been visited by inspectors trained in mosquito extermination, and secondary cases have been so far practically unknown. As a result of a partial malarial survey of the city, it has been found that the disease is principally localized along the railroad and the railroad yards. Further investigations along this line are now under

way. The work has advanced far enough to demonstrate that there is very much less malaria now than is usual at this time of the year; the Mexican physicians are unanimous in stating that the amount of *paludismo* is now very small.

The other group of diseases which have been brought under control are the dysenteries and diarrhoeas, and the preventive measures which seem to be directly responsible for the improvement are the following: the suppression of flies and the protection of foodstuffs in the markets by screening; the improvement in the milk-supply, and disinfection and isolation of dysenteric cases. The number of milk-venders in the city is approximately 150, and 200 samples of milk have been examined for dirt, adulteration and the percentage of fat. The milk examinations are made at irregular intervals on unannounced dates, each vender's milk being examined at least quarterly. The measure, however, which seems most directly responsible for the diminution in the number of cases and deaths from intestinal diseases is the antifly campaign. The city water has been frequently examined in the laboratory and found uniformly good. No cases of yellow fever have originated in Vera Cruz or been brought to the port.

FOREIGN STUDENTS AND THE UNITED STATES

Dr. P. P. Claxton, United States commissioner of education, has authorized the preparation and publication of a special bulletin describing, for the use of foreign students, the facilities for professional and collegiate study in higher institutions of learning in this country. The bulletin will be printed in several languages. "This is America's opportunity," writes Commissioner Claxton. "Thousands of students who have been attending universities in Europe will be obliged to look elsewhere for higher education, not only this year, but perhaps for years to come. Many foreign students are already coming to us, many more will come as the result, direct and indirect, of present events. We have now a supreme opportunity to demonstrate our capacity for intellectual leadership. Whether the war continues three months or three years, our opportunities and obligations to take the lead in education and civilization will be the same, and America should respond by offering the best opportunity in the world for her own students and for those who may come from other countries. In the case of South America this student migration will be facilitated by the opportune opening of the Panama Canal. Students from the western coast of South America will find it alluringly convenient to go via Canal to educational centers in the United States. Within the last two decades the increase in opportunity for graduate study and research, and for professional and technical education has been very remarkable, much greater than most people even in America realize. The recent raising of standards and the better equipment of medical schools, the large endowments and appropriations for all forms of engineering, the marvelous growth of our colleges of agriculture, the development of colleges and schools of education, and the rapid increase in income of all the better colleges make it possible for this country to take the lead in education in a way that would have been impossible even at the beginning of the century."

BOTANISTS OF THE CENTRAL STATES

In accordance with a vote taken at the Cleveland meeting of the American Association for the Advancement of Science, it is determined to reorganize the Botanists of the Central States. A very large majority of the members of the organization, either by letter or by personal statement at the Cleveland meeting, have expressed their desire for a resumption of the meetings of the organization, especially in years in which the Botanical Society of America meets outside of the states which comprise our territory. Since the last meeting of the Botanical Society of America was at Atlanta, and the next meeting is to be at Philadelphia, the present year seems especially favorable for a meeting of the Botanists of the Central States. I am able to announce that we have a very cordial invitation to hold

our next meeting at the Missouri Botanical Garden, in connection with the twenty-fifth anniversary celebration of the Garden. This celebration will be held Thursday, October 15, and Friday, October 16. It is planned to have a meeting of the Botanists of the Central States for the reading of papers and the transaction of business on Saturday, October 17. It is believed that this meeting, combined as it is with the very important celebration of the Botanical Garden, will be one of the most important meetings of American botanists within recent years.

Members desirous of presenting papers at this session should send to the undersigned as soon as possible the titles of such papers, indicating also the time and facilities needed for their presentation. Such titles must be in the hands of the undersigned by October 1, since it is the intention to mail the final program of the meeting by October 5.

Through the courtesy of the director, Dr. George T. Moore, it is learned that the Botanists of the Central States are to be the guests of the Missouri Botanical Garden at luncheon on Saturday, October 17.

HENRY C. COWLES,
Secretary

THE UNIVERSITY OF CHICAGO,
September 10, 1914

SCIENTIFIC NOTES AND NEWS

DR. FRIEDERICH VON MÜLLER, professor of medicine at Munich, has been elected rector of the university for the year 1914–15.

DR. GUSTAV A. SCHWALBE, professor of anatomy at Strassburg, celebrated on August 1, his seventieth birthday.

THE sixth session of the Macbride Lakeside Laboratory has closed after a successful summer's work. The teaching staff this year was as follows: In botany, Professor Thomas H. Macbride, acting director, James E. Gow, of Coe College, and A. F. Ewers, of McKinley High School, St. Louis; in geology, J. E. Carman, University of Cincinnati; in zoology, T. C. Stephens, Morningside College, and Wayne Hagan, Clinton High School. L. H. Pammel, of Ames, and B. H. Bailey, of Coe College, gave special courses of lectures.

THE American Fisheries Society will hold its forty-fourth annual meeting in Washington, D. C., from September 30 to October 3, at the new National Museum building. The program includes papers on aquatic biology, parasites and diseases of fishes, utilization of fisheries products, fish culture and commercial fisheries. The society numbers over 700 members. Professor Henry B. Ward, of the University of Illinois, is president, and Professor Raymond C. Osburn, of Columbia University, secretary.

THE British Board of Agriculture and Fisheries has awarded research scholarships in agricultural and veterinary science of the annual value of £150, tenable for three years, as follows: *Agricultural science,* J. Ll. Evans (Wales), S. M. Wadham (Cantab.), J. W. Munro (Edinburgh). *Veterinary Science,* R. Daubney, A. H. Adams. The board has also awarded Mr. E. W. Jeffreys (Wales) an agricultural scholarship tenable for two years to fill a vacancy.

THE president of the British Board of Trade has appointed a committee to consider and advise as to the best means of obtaining for the use of British industry sufficient supplies of chemical products, colors and dyestuffs of kinds hitherto largely imported from countries from which they can not now be obtained. The Lord Chancellor will be chairman of the committee, and the following is a list of the other members: Dr. George T. Beilby, F.R.S., Dr. J. J. Dobbie, F.R.S., Mr. David Howard, Mr. Ivan Levinstein, Professor Raphael Meldola, D.Sc., F.R.S., Mr. Max Muspratt, Professor W. H. Perkin, Ph.D., D.Sc., F.R.S., Mr. Milton Sharp, Sir Arthur J. Tedder, Mr. Joseph Turner, Mr. T. Tyrer, together with Mr. John Anderson, of the National Health Insurance Commission, and a representative of the Board of Trade. The secretary of the committee is Mr. F. Gossling of the Patent Office.

DR. H. FLOURNOY, resident psychiatrist at the Henry Phipps Psychiatric Clinic of Johns Hopkins Hospital and a member of the medical reserves of the Swiss army, has left Baltimore to return to Switzerland, in answer to the call for reservists.

Harry A. Curtis, assistant professor of chemistry at the University of Colorado, has returned after a year's leave of absence, during which time he took graduate work in chemistry at the University of Wisconsin, receiving the degree of doctor of philosophy.

Mr. George H. Chapman has resumed his duties as assistant botanist at the Massachusetts Agricultural Experiment Station after a year spent at the University of Prague with Dr. F. Czapek.

Dr. William J. Milne, president of the New York State College of Teachers in Albany and author of mathematical text-books, died on September 4, at the age of seventy-one years.

Dr. Béla Haller, associate professor of zoology at Heidelberg, has died at the age of fifty-six years.

UNIVERSITY AND EDUCATIONAL NEWS

Several citizens of Toronto have agreed to contribute $15,000 for five years to enable the University of Toronto to increase its research work.

The will of Mrs. Josephine A. Binney gives $10,000 to the Women's College of Brown University.

The Henry S. Denison Memorial Building, for Medical Research at the University of Colorado, has now been made ready for use. It contains laboratories for research in bacteriology, pathology, physiology, chemistry and clinical methods.

It is believed that in Oxford and Cambridge the number of undergraduates in residence next term will be reduced by one half.

Dr. R. M. Strong, of the department of zoology of the University of Chicago, has accepted the chair of anatomy at the University of Mississippi.

In the department of physiological chemistry of the Jefferson Medical College, Raymond H. Miller, B.S. (Pennsylvania State), and J. O. Halverson, M.S. (Missouri), have been appointed instructors. Martein E. Rehfuss, M.D. (Pennsylvania), after spending three years in study abroad, has been appointed research associate, and Olaf Bergheim has been promoted to be a demonstrator.

Maxwell Sillman, M.S., formerly instructor in physiological chemistry in Jefferson Medical College, has been appointed instructor in chemistry in the medical school of Baylor University, at Dallas, Texas.

Among appointments at the University of Montana are the following: L. S. Hill, assistant professor of mathematics and astronomy; Dr. Fred. H. Rhodes, of Cornell University, instructor in chemistry, and A. W. L. Bray, a graduate of Cambridge and London, instructor in biology.

Dr. H. C. Stevens, associate professor of psychology in the University of Washington, has been appointed associate professor of education in the University of Chicago.

A. Vincent Osmun, assistant professor in the department of botany of the Massachusetts Agricultural College, has been made associate professor, and F. A. McLaughlin, of the same department, has been promoted to the rank of instructor.

Dr. W. P. Gowland, of the University of Liverpool, has been appointed to the chair of anatomy at the University of Otago, Dunedin, New Zealand.

DISCUSSION AND CORRESPONDENCE

A RECENT CASE OF MUSHROOM INTOXICATION

Although it has been stated, in standard works on fungi, that a common and otherwise edible species (*Panæolus papilionaceus*) sometimes has intoxicating properties, it seems desirable to record the recent experience of two persons who ate considerable numbers of this species, unmixed with other kinds.

They were familiar with this species and various others, and had on several occasions eaten it in small numbers, mixed with other kinds, without noticeable effects. This is a small, rather delicate, umbrella-shaped mushroom, which is common on cultivated land, planted to farm crops.

Mr. W., whose narrative is here given, is a middle-aged, vigorous man, strictly temperate in his habits. He is a good botanist, and has made a special study of fungi. The account

of his experience was dictated to me by him about a week after the event, while fresh in his memory.

The lady referred to as Mrs. Y., who also ate the mushrooms, is his niece by marriage. Her husband (Mr. Y.) was present, but ate no mushrooms. He could observe some things not noticed by the victims, both of whom experienced nearly the same effects. Mrs. Y. also gave the writer a personal account of some of her symptoms, essentially the same as those here narrated. This article in its present form has been read by Mr. W. and approved by him.

The parties are natives of Oxford County, Maine, where the event occurred. Their real names are withheld, by request. The effects experienced are in some respects similar to those caused by hashish; others are like those experienced by some opium smokers, especially the multiplication of objects and their bright colors. The appearance of vivid colors recalls the symptoms described by Dr. Weir Mitchell, when he took Mexican mescal pills, as an experiment. The loss of the power of estimating time and distance, as in some dreams, is interesting, as existing when other faculties were active.

Narrative of Mr. W.

On July 10, 1914, I gathered a good mess of the mushrooms (*Panæolus papilionaceus*) and had them cooked for dinner. There may have been about a pound of them as gathered, but when fried in butter they made no great quantity, owing to their softness and delicate structure.

They were all eaten by Mrs. Y. and myself. Peculiar symptoms were perceived in a very short time. Noticed first that I could not collect my thoughts easily, when addressed, nor answer readily. Could not will to arise promptly. Walked a short distance; the time was short, but seemed long drawn out; could walk straight but seemed drowsy; had no disagreeable stomach sensations, effects seemed entirely mental; remember little about the walk. Mrs. Y. was in about the same condition, according to Mr. Y. My mind very soon appeared to clear up somewhat, and things began to seem funny, and rather like intoxication. Walked with Mr. Y. A little later objects took on peculiar bright colors. A field of redtop grass seemed to be in horizontal stripes of bright red and green, and a peculiar green haze spread itself over all the landscape. At this time Mrs. Y. saw nearly everything green, but the sky was blue; her white handkerchief appeared green to her; and the tips of her fingers seemed to be like the heads of snakes.

Next, say about half an hour after eating, both of us had an irresistible impulse to run and jump, which we did freely. I did not stagger, but all my motions seemed to be mechanical or automatic, and my muscles did not properly nor fully obey my will. Soon both of us became very hilarious, with an irresistible impulse to laugh and joke immoderately, and almost hysterically at times. The laughing could be controlled only with great difficulty; at the same time we were indulging extravagantly in joking and what seemed to us funny or witty remarks. Mr. Y., who was with us, said that some of the jokes were successful; others not so, but I can not remember what they were about.

Mr. Y. says that at this time the pupils of our eyes were very much dilated, and that Mrs. Y. at times rolled up her eyes and had some facial contortions, and slight frothing of saliva at the mouth. Later we returned to the house, about one quarter of a mile. At this time I had no distinct comprehension of time; a very short time seemed long drawn out, and a longer time seemed very short; the same as to distances walked; though not so when estimated by the eye. The hilarious condition continued, but no visual illusions occurred at this time.

After entering the house, I noticed that the irregular figures on the wall-paper seemed to have creepy and crawling motions, contracting and expanding continually, though not changing their forms; finally they began to project from the wall and grew out toward me from it with uncanny motions.

About this time I noticed a bouquet of large

red roses, all of one kind, on the table and another on the secretary; then at once the room seemed to become filled with roses of various red colors and of all sizes, in great bunches, wreaths and chains, and with regular banks of them, all around me, but mixed with some green foliage, as in the real bouquets. This beautiful illusion lasted only a short time. About this time I had a decided rush of blood to my head, with marked congestion, which caused me to lie down. I then had a very disagreeable illusion. Innumerable human faces, of all sorts and sizes, but all hideous, seemed to fill the room and to extend off in multitudes to interminable distances, while many were close to me on all sides. They were all grimacing rapidly and horribly and undergoing contortions, all the time growing more and more hideous. Some were upside down.

The faces appeared in all sorts of bright and even intense colors—so intense that I could only liken them to flames of fire, in red, purple, green and yellow colors, like fireworks.

At this time I began to become alarmed and sent for the doctor, but he did nothing, for the effects were wearing off when he came. Real objects at this time appeared in their true forms, but if colored they assumed far more intense or vivid colors than natural; dull red becoming brilliant red, etc. A little later, when standing up, I had the unpleasant sensation of having my body elongate upward to the ceiling, which receding, I grew far up, like Jack's bean-stalk, but retained my natural thickness. Collapsed suddenly to my natural height.

At this time I noticed the parlor organ and tried to play on it, to see the effect, but could not concentrate my mind nor manage my fingers. About this time my mind became confused and my remembrance of what happened next is dim and chaotic. Probably there was a partial and brief loss of consciousness. Laid down to wait for the doctor. Looking at my hands, they seemed to become small, emaciated, shrunken and bony, like those of a mummy. Mrs. Y. says that at this time her hands and arms seemed to grow unnaturally large.

When I attempted to scratch a spot on my neck, it felt like scratching a rough cloth meal-bag full of meal, and it seemed as large as a barrel, and the scratching seemed quite impersonal. Later I imagined I was able, by a sort of clairvoyance, to tell the thoughts of those around me. Soon after this our conditions rapidly assumed the very hilarious phase, similar to that of the early stages, with much involuntary laughing and joking. This condition gradually diminished after three o'clock, until our mental conditions became perfectly normal, at about six o'clock P.M. The entire experience lasted about six hours. No ill effects followed. There was no headache, nor any disturbance of the digestion.

A. E. VERRILL

YALE UNIVERSITY

SCIENTIFIC BOOKS

Plane Trigonometry and Applications. By E. J. WILCZYNSKI, Ph.D., University of Chicago. Edited by H. E. SLAUGHT, Ph.D., University of Chicago. Boston, New York and Chicago, Allyn and Bacon. 1914. Pp. xi + 265.

Elementary Theory of Equations. By L. E. DICKSON, Ph.D., University of Chicago. New York, John Wiley & Sons. 1914. Pp. v + 184.

Among the prominent features of the former of these two elementary text-books is the fulness of its explanations of fundamental processes. In fact, it might at first appear that nothing was left for the teacher to explain, but the numerous illustrative examples and problems should serve to awaken discussion and to enliven the recitation periods. The clearness with which the fundamental ideas are developed tends to make the book unusually easy for the student.

The book is divided into two nearly equal parts. The first part is devoted to the solution of triangles, and is published separately for the use of secondary schools. In this part practical applications to surveying are emphasized, and the use of the slide rule and the logarithmic tables are clearly exhibited. The

author's extensive experience as a practical computer, combined with his keen mathematical insight, have enabled him to provide against the usual difficulties and pitfalls which beset the path of the beginner in this field.

The second part treats the more advanced parts of elementary trigonometry together with applications to simple harmonic curves, simple harmonic and wave motion, and harmonic analysis. The two parts are intended to cover the work in plane trigonometry usually given during the freshman course in the colleges. Notwithstanding the unusually large number of trigonometries which are now on the market, this book seems to have important characteristic properties.

From the standpoint of pure mathematics plane trigonometry may be of comparatively little importance, but it occupies a strategic point in the mathematical training of most students who take freshman mathematics in our colleges and universities. The numerous direct applications of this subject and the training which it provides for a wise use of approximate results combine to make it especially important that the student should have clear views at this point in order that mathematical thinking may become natural to him. Professor Wilczynski's book seems to guard to an unusual degree against vague or incorrect impressions.

Professor Dickson's *Elementary Theory of Equations* relates to a subject where both textbooks and students are much less numerous than in the subject considered above. The classic work by Burnside and Panton, in two volumes, is too extensive for the available time in many institutions. Moreover, it omits the important subject of systems of linear equations. Some of the more recent works aim to lead up to modern theories too rapidly to give enough room to the classic fundamental theories.

In the present book the author has provided for two courses by marking with a dagger many of the more difficult sections which could be omitted without breaking the continuity of the course. The aggregate of the sections thus marked is more than fifty pages, and the rest constitutes a very brief course in this subject. A large number of illustrative problems are solved in the text, and about five hundred graded exercises are distributed through the various chapters. To the reviewer the book appears to excel all others extant for a first course in this subject.

As might be expected, the author has paid especial attention to rigor and conciseness in presentation, and has made a wise selection from the vast amount of material relating to the subject in hand. His masterful skill in reaching the essential points by the most direct means is everywhere apparent. In addition to a treatment of the rational integral function in one unknown, the book contains a good introduction to the theory of determinants and the solution of a system of simultaneous linear equations.

For the sake of simplicity very few modern concepts are introduced. The Galois theory is entirely omitted and the subject of invariants is only illustrated by a few examples. The concept of rank of a determinant is introduced but the closely related concepts of matrix and rank of a matrix are not developed. The introduction of these concepts would have enabled the author to state more concisely some results relating to a system of linear equations.

G. A. MILLER

Rubber and Rubber Planting. By R. H. LOCK, Sc.D. London, Cambridge University Press; New York, G. P. Putnam's Sons. 1913. Pp. 13 and 245. 5 by 7 inches.

The purpose of the author of this book has been to present an introductory outline of the subject, as stated in the title, to meet the needs of as wide a circle of readers as possible. One can not but feel that the result would have been more satisfying if the limitation of the size of the volume had not prevented the author from doing what he really wished to do. A better end could perhaps have been gained by confining the treatment to the most important rubber plant, economically regarded, *Hevea Braziliensis.* Had this been done, the least satisfactory chapters (II., X. and XI.),

scarcely more than summarily encyclopedic in their character, would have been omitted with little damage to the whole, and would have been more than compensated for by a still fuller treatment of the behavior of the *Hevea* tree under cultural conditions, a subject with which the author is familiar because of residence in Ceylon and intimate study of its plantations. Indeed, a complete presentation of his studies of latex flow and methods of tapping, bringing the whole of his work in one volume, would have been distinctly valuable to the planter and as much of the book is occupied by details which, in spite of the purpose of its author, are beyond the scope of, or insufficient for, the general reader, the only disadvantage would be found in a perhaps smaller market. The prospective planter, and, still more so, the person who still entertains the notion that rubber planting is a road to immediate wealth, will find plenty of material for an introductory study of the situation as regards rubber planting in the east; and if he has actually started on the venture, plenty of suggestion, of great value from the practical point of view. So that, while the reviewer thinks that the interests of a wide circle of readers have been misapprehended, and ill met, the book is most decidedly a good general introduction to the study of the problem of *Hevea* cultural methods in the far east, and would have been still more useful had the subject been extended and a fuller bibliography appended.

It would also have added not a little to the text in point of value to the intelligent student to have given specific citations of authorities on which the author frequently and properly depends, while a little further consultation of these would have obviated some minor insufficiencies and errors, as, for example, that made when it is stated that the methods of preparing guayule rubber are kept secret.

Plantation rubber has received its apotheosis, and it is with us to stay. The doom of wild pará, to say nothing of the inferior kinds, is as sure to sound as has that of guayule. No two economic plants have histories more full of romance than these, but, as those of early history in general, exploration, adventure and exploitation of wild rubber fields must give way to plain, work-a-day methods. Civilized man does not hunt for his acorns and roots; he grows them. No more can he afford to hunt for his rubber; this also must he raise intensively and systematically, reducing costs and perfecting the product by the help of every scientific method at his command. In reading anew the history given by Mr. Lock of the attempt, now happily completely successful, to introduce the *Hevea* into the east, one's admiration of the pluck and faith displayed by the British, to whom everlasting credit must be rendered for their service, is again awakened. If Kew had done nothing more for civilization than this, the rubber producers of the far east could well take the support of that institution on their own shoulders for all time, and still never repay the debt. Botanical science needs the support of the business man more than he is willing at present to render. It is not inappropriate to say this at this moment when the big rubber companies are occupying the field. There are still new sources of wealth for science to search for, but science must work in its own way. We should like to see the man of business willing to take the long chance in the interests of science with the same *sang froid* as in the interests of business. He will be the gainer in the end.

Mr. Lock's account of the physiology of latex flow is valuable, but, at the same time, it shows us how far we are yet from having more than a very meager understanding of the whole subject. In this, the way to an accurate scientific study of the physiology of rubber secretion has been blazed out by the more immediately necessary practical tests so that the planter might have real guidance in handling the tree. The nature of the "wound response" characteristic of *Hevea*, is still to be closely studied. Here it may be remarked that the relation of yield to water-supply appears also to be antithetic to that observed in *Castilloa* and the Guayule (*Parthenium argentatum*), since the total yield and highest percentage of rubber in *Hevea* varies directly with the water

available (at least within the limits observed), according to our author, while the reverse is true in the other species just mentioned. Mr. Long might profitably have referred to Dr. Spence's experiments on *Hevea* in connection with his discussion of the functions of latex, about which we are indeed, as he states, very much in the dark.

No less important practically is the question of the nature of coagulation, and here also from now on careful scientific methods must be employed if further material progress is to be made.

Mr. Long's book indicates these and numerous other problems which await the attention of both planter and scientist, and because of this and because it contains a summary of practical results in plantation methods and management thus far obtained stated by an evidently careful student of practical methods, it will be worth study by every one interested. Tables of approximate costs and of data derived from tapping experiments based upon his actual experience in the east are given and the value of these is beyond question as offering guidance to those concerned.

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THE WORK OF THE U. S. FISHERIES MARINE BIOLOGICAL STATION AT BEAUFORT, N. C., DURING 1913

THE laboratory of the Bureau of Fisheries at Beaufort, North Carolina, was opened to investigators engaged in the scientific and economic problems of the Bureau and to independent workers on June 9, and closed about the middle of September. The number assigned to the laboratory taxed its capacity and not all applicants could be accommodated. Following is a brief summary of the summer's work and of the various activities of the station during the year.

The equipment of the station was enhanced by the addition of beam trawls, a small fish trawl, stow-net, new pound-net, three new rowboats, a photomicrographic outfit, and numerous other articles needed in the laboratory, power house and mess house. The most important addition was that of a 33-foot motor boat equipped with a 24 horse power 4-cycle 4-cylinder Lamb engine. This boat has a 10-foot saloon with suitable accommodations for extended trips and a large after deck, convenient for landing the beam trawls, boat dredges and fish trawls used at the station. It is a one-man control boat and is especially adapted to the needs of the laboratory. A new dark room for photographic work was built in one end of the laboratory. This replaced the one on the museum floor and added greatly to the conveniences of the laboratory.

The success attendent on the propagation of the diamond-back terrapin at this station has attracted considerable attention and a number of persons are contemplating engaging in this industry. Early in the year a company was formed at Beaufort and plans were perfected for growing terrapin for market on a large scale. The company has a well-equipped establishment with over 3,000 terrapin purchased for breeding purposes. The adaptability of this form to artificial conditions was shown by the fact that terrapin purchased during the laying season continued their activities in captivity and before the close of the season over 700 young terrapin had been added to the company's stock.

The 1913 brood of the laboratory numbered 1,424 on November 10. This is an increase of 198 over the brood of the preceding year. The average number of eggs per terrapin has steadily increased with longer periods of confinement. Those purchased in the early stages of the experiment, this year averaged over 13 eggs apiece. It was also quite evident from the number of eggs per nest that the terrapin in this pound laid twice during the season. In October, 554 terrapin belonging to the broods of 1911 and 1912 were planted in suitable localities in Lynnhaven Bay, Va., and 200 of the 1912 brood were sent to Chase, Florida, for experimental purposes. A brief account of the cultural experiments with this species by W. P. Hay and H. D. Aller is contained in Economic Circular No. 5 of the Bureau of Fisheries issued June 24, 1913, and entitled "Arti-

ficial Propagation of the Diamond-back Terrapin."

Following the plan outlined in 1912, special emphasis was laid on the collection of data on the fishes of the region. The date of spawning of the southern flounder (*Paralichthys lethostigmus*) was determined. As the flounder fishery is an important one and as the various edible species are less abundant than formerly, steps are being taken to engage in their propagation at this station. The opportunities for engaging in propagation work have been advanced by the addition of the position of fish-culturist, and Mr. Charles Hatsel, who showed a great deal of natural ability in carrying out the cultural experiments with the diamond-back terrapin, fills this position.

For the purpose of determining the location, extent and resources of the off-shore fishing grounds and to encourage their development, the Fisheries Steamer *Fish Hawk* was detailed to the laboratory for a period of two months, and on September 6 began a brief survey. A number of grounds where blackfish or sea bass (*Centropristes striatus*) were abundant were surveyed and charted, and representative collections of the local fauna were made. The success attendant on line fishing by members of the *Fish Hawk's* crew and of fishermen visiting these grounds are encouraging, and more than 15,000 pounds of this fish were taken. A brief summary of the results of this work is contained in Economic Circular No. 8, of the Bureau of Fisheries, issued February 25, 1914, and entitled "The Offshore Fishing Grounds of North Carolina."

The following species taken in the Beaufort region during the year are believed to be new records for the coast of North Carolina:

Anchovia argyrophana (Cuvier & Valenciennes),
Anchovia perfasciata (Poey),
Aprionodon isodon (Müller & Henle),
Blennius stearnsi Jordan & Gilbert (?),
Calamus calamus (Cuvier & Valenciennes),
Callionymus calliurus Eigenmann & Eigenmann,
Congermuræna balearica (De la Roche),
Clupea hanengus Linnæus,
Hemicaranx amblyrhynchus (Cuvier & Valenciennes),
Ioglossus calliurus Bean,
Letharchus velifer Goode & Bean,
Ogcocephalus radiatus (Mitchell),
Pagrus pagrus (Linnæus),
Parexocœtus mesogaster (Bloch),
Platophrys ocellatus (Agassiz),
Rhomboplites aurorubens (Cuvier & Valenciennes),
Rypticus bistrispinus (Mitchill),
Syacium micrurum Ranzani,
Vulpecula marina Valmont.

A report on the sharks and rays of the Beaufort region, in which special stress is laid on the character of the teeth and dermal denticles as an aid to identification, is being prepared by the director.

The scientific workers at the laboratory have furnished the data on which the following brief summary of their work is based:

Dr. C. H. Edmondson, of Washburn College, devoted six weeks to a survey of the marine protozoan fauna in the vicinity of Beaufort. This work was conducted along three more or less closely connected lines, as follows:

1. Study of pelagic forms obtained by means of tow nets and the stow net. The latter was used to good advantage in collecting the free-swimming surface forms.

2. Dredgings taken in the vicinity of the sea-buoy on a bottom of thick black mud in 5 or 6 fathoms of water. This proved to be very rich in Foraminifera.

3. Examination of the contents of the stomachs of a number of species of fishes with a view of determining whether or not certain marine protozoa might be considered as constituting a portion of the food of these fishes.

The similarity of the protozoan fauna of the Beaufort region in many of its features to that found in such widely separated localities as Woods Hole, Mass., the Dry Tortugas, the Pacific Ocean off the coast of southern California and even in the Puget Sound region, was striking.

Of the fishes examined for their stomach contents, only three species showed evidences

of having ingested protozoa. Many species of protozoa were always found in the stomachs of the hairy-back or thread herring (*Opisthonema oglinum*) and the menhaden (*Brevoortia tyrannus*). *Dinoflagellates* were commonly present in the stomachs of these species, and several species of *Ceratium* and *Peridinium* were always present, while *Dinophysis* was nearly as constant.

Tintinnopsis, a ciliate, represented by at least three species, was always present in the menhaden, frequently in myriads. The pinfish (*Lagodon rhomboides*), also a surface feeder, ingests masses of algæ and probably protozoa, but only rarely were evidences of the latter found. Broken fragments of *Ceratium* were occasionally present in its stomach. The coarse gill-rakers of this fish probably permit the minute organisms to escape.

Tunicates were also examined and the ciliate, *Tintinnopsis*, was almost universally found in the digestive cavities and may be considered one of the items of food of these forms.

A more extensive study of this phase of the problem would undoubtedly verify the belief that the protozoa are of very high economic value as food, either directly or indirectly, for many fishes as well as other marine forms.

Mr. W. C. George, of the University of North Carolina, an independent worker, devoted considerable time to a general study of the local fauna and in studying early stages in the embryology of *Chætopterus* and the reduction phenomena exhibited in the medusæ of *Pennaria*.

Professor W. P. Hay, of Washington, D. C., continued the investigation of the diamond-back terrapin and gathered additional data for a final report in which, it is hoped, a complete account of the life history of this animal can be given. Observations were made on the rate of growth of the loggerhead turtle. Two of the young ones hatched at the laboratory in September, 1912, survived the winter and attained the age of one year in captivity. During this time these increased in length from about 77.3 mm. to 200 and 218 mm. respectively.

Rather late in the season an investigation of the blue crab was begun, to determine rate of growth and interval between moults. From the results obtained it appears that the species casts its shell with fair regularity about every two weeks and increases in measurement about one quarter to two fifths at each moult up to maturity. After sexual maturity has been attained moulting probably ceases and the animals, especially the females, fall easy victims to various parasites. It was not possible to secure positive evidence that the females lay a second lot of eggs, though it is probable that those which survive the winter do so.

Aside from the experimental lines of work considerable time was devoted to collecting, identifying and describing the decapod crustaceans of the region. A paper embodying the results of this work is being prepared.

Mr. Selig Hecht, of the College of the City of New York, who was assigned to the director for duty, accompanied certain of the collecting trips and engaged in (1) a preliminary study of the rate of growth of the menhaden; (2) the relation of form, weight, length and other body measurements for the following species: *Anchovia brownii, Anchovia mitchilli, Brevoortia tyrannus, Leiostomus xanthurus, Peprilus alepidotus* and *Orthopristis chrysopterus*. In all cases there was a clear correlation between weight and length, so that weight was shown to be a cubic function of length. The interrelationships of the various parts of the body and total length were established for each species. It was found for each that the form of the fish remained constant throughout the life of the individual, and correlated with this that the volume of the fish was a function of the product of the length, width and depth, as well as a function of the weight. These combined relationships mean that weight in these species is equal to the product of length, width and depth times a constant which differs for each species.

The relative growth of various parts of the fish was studied, and the results show that although apparently the various parts of the fish grow at different constant rates relative to the

total length, their absolute rates of growth are the same. This apparent rate of growth of each part relative to the total length is a function of the ratio of the length of the part to the total length of the fish. (3) Study of the puffing mechanism of *Spheroides maculatus*. Anatomically the mechanism is composed of two parts: a diverticular bag from the ventral wall of the esophagus, and several sphincter muscles. In inflation with water, the liquid is drawn in as in a normal inspiration. During the expiration, however, all openings to the exterior are shut and the muscle controlling the opening into the esophagus, and eventually into the diverticular bag is released and the water is forced into the diverticulum or "puff-bag." This occurs several times before inflation is complete. Inflation with air differs from that of water in that during inspiration the air is drawn into the oral cavity largely through the opercular openings and not through the mouth.

Dr. Albert Kuntz, of the University of Iowa, continued the investigation of pelagic fish eggs and larvæ. This work was undertaken for the purpose of securing a record as complete as possible of the time of spawning and of the embryological and larval development of fishes with pelagic eggs breeding in these waters during the summer, one of the primary objects being to afford a ready means of identifying either eggs or larval fishes at any time during embryological and larval life.

Pelagic eggs and larvæ of not less than eight species were taken during the summer. Complete records of the embryological and larval development of two species, viz., *Bairdiella chrysura* (Lacépède) and *Anchovia mitchilli* (Cuvier & Valenciennes) were secured. Observations on the eggs and larvæ of the remaining species as yet remain incomplete.

Dr. S. O. Mast, of Johns Hopkins University, devoted his time to a study of the changes in pattern and color in fishes, especially the flounders. The flounders lie on the bottom most of the time and the skin assumes a color and pattern so nearly like that of the environment that it is frequently very difficult to see them. On a black bottom they become black; on a white bottom, white; on a yellow bottom, yellow; on a blue bottom, bluish; on a red bottom, reddish, etc. All of these changes in the skin are regulated through the eye. This indicates color vision. If the bottom is finely mottled, the pattern on the skin assumes a fine grain; if coarsely mottled, it assumes a coarse grain; but there is no evidence indicating an actual reproduction of the configuration of the background.

If, after the skin has become adapted to a given bottom, the fish are removed to a different bottom, they tend to return to the original, *i. e.*, they tend to select a bottom which harmonizes with their skin. A large number of photographs and autochromes were made to facilitate this study and to serve in illustrating the report.

Mr. L. F. Shackell, of St. Louis University school of medicine, continued work begun in the summer of 1912 on the protection of wood against the attacks of marine borers. The test employed on the pieces of wood treated at that time—submersion in the water of Beaufort Harbor for ten months—eliminated many forms of treatment. The work of the past summer, therefore, consisted of detailed experimental work involving a very few materials found to be effective over a relatively short period. The cost of treatment was also considered in the later work. It is not expected that definite results of any economic importance will be obtained in less than three years' time, during which the present series of treated pine poles will be continually submerged under sea water.

Mr. H. F. Taylor, of Trinity College, continued his investigations of the scales of fishes, (1) concluding as far as possible the investigations of the squeteague scales and (2) verifying and amplifying the results thus attained by similar work on other species.

On account of its adaptability to artificial conditions the pig-fish (*Orthopristis chrysopterus*) was used. Scales of over 80 specimens were examined and the growth rate calculated as for the squeteague. Evidences thus found point to the first year as that of sexual maturity.

While the scales of the pig-fish are much more regular in their features than those of the squeteague, observations of the radii corroborate the evidence obtained in 1912 that the radii are merely fissures to permit greater freedom of body movement.

Dr. H. V. Wilson, of the University of North Carolina, spent the summer in an examination of the collection of Philippine sponges. The collection embraces all the great groups of sponges: *Calcarea, Hexactinellids,* Tetractinellids including *Lithistida, Monaxonida* and *Keratosa.* Sixty-odd packages were examined. These were found to represent twenty-five species, the majority of which are new forms.

Dr. James J. Wolfe, of Trinity College, devoted his investigations primarily to an examination of the Diatomaceæ of Beaufort. Extensive tow-net collections were made at various localities under a variety of conditions. These are to be continued at monthly intervals for one year. By this means it is hoped a thoroughly representative collection will be secured. Mounts have been made of about 200 species and considerable progress has been made in their identification.

The culture of *Padina* sporelings was again carried on—now with special reference to parthenogenesis. Cultures demonstrably parthenogenetic, started in the laboratory, as in the earlier work, were transferred to the sea. Unfortunately these were destroyed by the severe storm of September 3–4, necessitating their repetition before this work can be reported in full.

Mr. Raymond B. Beckwith, of Olivet College, and Mr. Francis Harper, of Cornell University, who were assigned to the director for duty, accompanied the various collecting trips and kept complete records of their observations, devoting special attention to the habits of the fishes of the region.

In addition to his other duties, Mr. Beckwith accompanied the *Fish Hawk* on the various collecting trips and assisted the director on the survey of the off-shore fishing grounds.

In addition to his regular work Mr. Harper took a large series of photographs and a number of autochromes of live flounders to be used in illustrating Dr. Mast's report. He also made numerous observations on the birds of the region. In addition to the incidental observations on field trips for fishes, a few holidays and Sundays were devoted to this work and a list of 87 species recorded. Ten birds were tagged with leg-bands furnished by the American Bird Banding Association. A breeding colony of herons on an island in the vicinity of the laboratory was found on August 9 to contain approximately 350 little blue herons (*Florida cærulæa*), 150 Louisiana herons (*Hydranassa tricolor ruficollis*), 8 black-crowned night herons (*Nycticorax nycticorax nævius*) and 6 American egrets (*Herodias egretta*). The little blue heron is not recorded as a breeding bird of North Carolina in the American Ornithologist's Union List, and this is the first time the American egret is known to have nested in the vicinity since 1899. Tentative arrangements have been made for the protection of the colony next year by a warden of the National Association of Audubon Societies. A number of species of shore birds were studied and photographed during the latter part of the summer.

An artist, Mrs. E. Bennett Decker, of Washington, D. C., was engaged in making the drawings to illustrate the embryological papers of Dr. Kuntz, and a series of drawings of the dermal denticles and teeth of the sharks of the region to accompany the report of the director on this subject.

LEWIS RADCLIFFE

BUREAU OF FISHERIES,
WASHINGTON, D. C.,
March 13, 1914

SPECIAL ARTICLES

THE TRANSMISSION OF TERRESTRIAL RADIATION BY THE EARTH'S ATMOSPHERE IN SUMMER AND IN WINTER

AN indirect measurement of the transmission through the earth's atmosphere of those radiations which are emitted by the earth's solid surface may be made by comparing the actual radiation of a surface at the terrestrial temperature toward the sky, with that toward a black body at absolute zero. The latter can

not be directly observed, but may be obtained by measuring the radiation toward a black surface of cavernous shape at known temperatures, using Stefan's law. This method has been used in the present research. If no radiation from the earth's surface can penetrate the atmosphere, as in the case when the sky is obscured by thick clouds, then the sky has the same effective temperature as the ground; but according as more or less surface radiation escapes to space through the air, the effective or apparent temperature of the sky diminishes, although never reaching absolute zero, for that would mean complete absence of absorption.

The radiation to the sky, measured either by a bolometer or a thermopile, falls off very slightly as the pointing of the instrument departs from the zenith, but more rapidly near the horizon where, if the blue is of inferior purity, the whitish sky has an effective temperature almost identical with that of the earth's surface. The dulling of the blue of the sky near the horizon is due to the dust and haze of the lower atmosphere.

its purity much better and a larger transmission, such as that indicated by the upper curve of Fig. 1 (which, however, is meant for a winter curve at sea level) may be given for terrestrial radiation emanating at inclinations with the horizon which are taken as abscissæ, the ordinates being the transmission. In cold winter weather when the ground is covered with snow, atmospheric dust is greatly diminished, and the conditions at sea level approximate to those on mountains. These transmission curves for variously inclined rays have been obtained by dividing the observed sky radiation by the unobstructed radiation to space appropriate to the observed temperature, assuming that space is at the absolute zero of temperature. The lower curve is founded on excellent observations, but the upper one is not so reliable and its shape is partly inferred. The maximum ordinate, however, is sometimes exceeded with the purest skies of winter, and at this season these uniform and deep blue skies are not rare, the difficulty in observing them being usually an instrumental one arising from the necessity

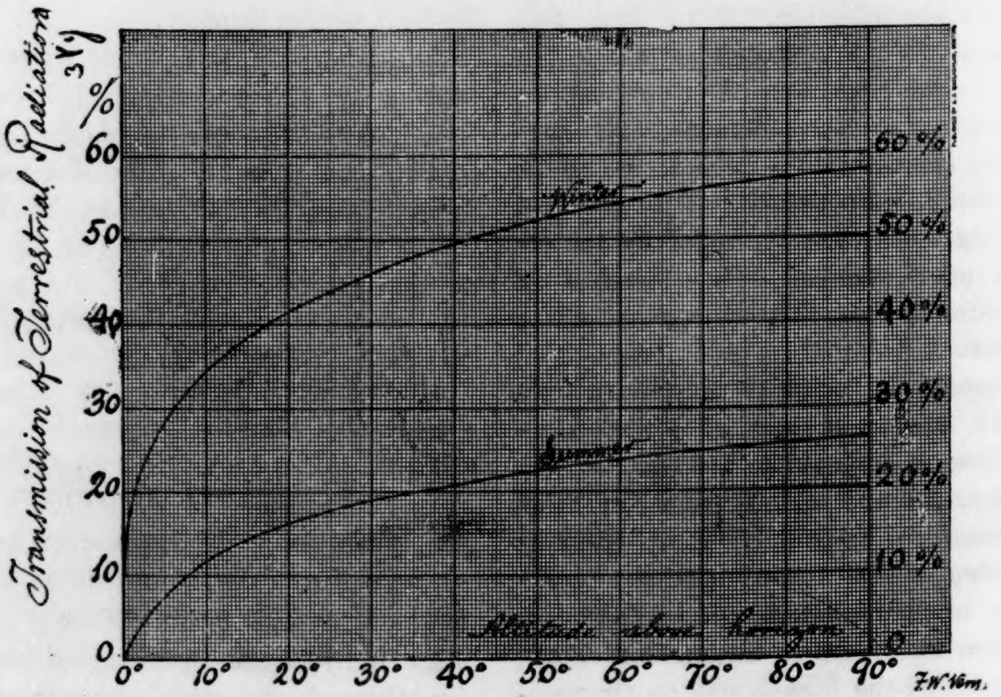

Fig. 1.

The lower curve of Fig. 1 is derived from observations of sky radiation on a summer's day of good blue sky near sea level. From mountain summits lifted above a large part of the atmospheric dust, the cerulean blue retains

of working with exposed instruments. The adopted values of summer transmission, T_S, and of winter transmission, T_W, from which the curves are drawn, are given in the next table:

Altitude Above Horizon	10°	20°	30°	40°	50°	60°	70°	80°	90°
T_s	.116	.162	.190	.208	.226	.238	.249	.256	.262
T_w	.344	.415	.459	.495	.525	.543	.560	.572	.581

Ordinarily when we speak of the transmissivity of the atmosphere without further specification, we mean the vertical transmission through the entire atmosphere. This can be obtained from the zenithal sky radiation, which is all that needs to be considered in what follows.

The transmission of terrestrial radiation by the atmosphere relates entirely to rays of long wave-length, and is effected by processes quite different from those which govern the transmission of solar radiation. Large variations may occur in the transmission of terrestrial radiation, and indeed quite suddenly, so that the eye scarcely appreciates the approach of new atmospheric conditions from any change in the appearance of the sky. It is only rarely that moderately smooth transmission curves can be obtained from the zenith to the horizon, because the conditions are apt to change before the observations can be finished.

The transmission of soil radiation to space is continually fluctuating between zero and an upper limit which seldom exceeds 60 per cent. of the maximum theoretical value for unimpeded radiation. At night, the diurnal convection diminishes greatly, and on land the wind is apt to fall as the sun goes down. Hence on many nights the surface air is approximately calm, and a thin quiescent layer of air forms in contact with the soil, in which the temperature is apt to fall below the dew point as a result of nocturnal radiation. Such a layer of nearly saturated air close to the ground, though exceedingly shallow, develops an extraordinary absorptive power for infra-red radiation in broad diffuse bands as a result of the production of the hydrols, and these bands may eventually extend so widely as to produce practically complete obstruction of terrestrial radiation. An example of this is given in my paper on "Sky Radiation and the Isothermal Layer."[1] After some hours of cooling, further diminution of temperature is prevented in such cases by the obstruction offered by this very thin air layer; and if we compare the loss of radiation from the earth's surface by night and by day, the former is much the smaller on such nights as are here considered. In spite of a phenomenally clear sky, the temperature of the ground as morning approaches often remains almost stationary, partly from the giving up of latent heat of evaporation in the condensation of aqueous vapor, and partly from this increase in the absorptive power of moist air as saturation becomes imminent.[2] If, however, instead of observing the superficial radiation through this closely adherent air layer, we take the radiation from a surface surrounded by relatively dry air in the room of an observatory, and let this radiation pass out through an aperture either directly to the sky, or, as is more convenient, allow the rays to pass to the sky after reflection from a mirror, placed outside the aperture, but far enough above the surface of the ground to be above the layer of adherent soil-chilled air, very little difference is to be found in the transmission of radiation from sources at terrestrial temperatures whether measured by night or by day, such differences as exist being those which may always be expected from changing cloudiness, or from variations in the general conditions as to moisture, etc.

It is common for writers on terrestrial radiation to assume that the earth as a whole radiates at a mean temperature of about

[1] *American Journal of Science*, Vol. XXXV., April, 1913, pp. 377 to 378 and 380 to 381.

[2] Those who are much in the open air know that on frosty mornings in winter a much more comfortable temperature is experienced on passing from the open into woods. The friction of moving air against the innumerable stems of the forest-cover helps to retain the absorbent layers of moist surface air in the woodland, and escape of radiation is impeded. Under exceptional circumstances the excess of temperature in the woodland may reach 20° or 30° C. See G. A. Pearson, "A Meteorological Study of Parks and Timbered Areas in the Western Yellow-pine Forests of Arizona and New Mexico," *Monthly Weather Review*, October, 1913. *Cf.* especially Figs. 6 and 7 (pp. 1620 and 1621).

$+15°$ C., or 288° Abs., or if the temperature regions alone are considered, a temperature of $+10°$ C. $=283°$ Abs. is thought to be better; and this is done by accepting the mean air temperatures observed by meteorologists as if they were those of the soil. Ordinarily, this does no harm for places where the sun shines at a low angle, or where the wind is strong enough to make air and surface temperatures coincide. But when the sun shines at a high angle above the horizon, or in desert regions with light airs, the astrophysicist must know the temperature of the actual radiating surface which becomes far hotter than the air temperature. Even in regions by no means of a desert character, surface layers of fairly dry soil in summer and in the middle of the day may be 20° or 30° C. hotter than the shade temperature of the air as commonly observed; and surfaces of rock in sunshine and on calm days are still hotter. Even plant surfaces in sunshine, though much cooled by evaporation, are appreciably warmer than the air. Taking the currently adopted thermal equivalent of radiation, $\sigma = 7.9 \times 10^{-11} \frac{\text{gram cal.}}{\text{cm.}^2 \text{ min.}}$, a black body at 288° Abs. C. radiates 0.544, and one at 298° radiates 0.623 gram. cal./sq. cm. min. Hence an arid region whose surface is on the average 20° C. hotter than the assigned air temperature during the sunlight hours, will radiate 14.5 per cent. more than the ordinary supposition indicates. On the other hand, most surface material radiates less than a black body (for example, a silicate, such as glass, radiates 93 per cent. as well as lamp black which, in turn, radiates a little less than a truly black body); and since minute accuracy is not attainable, the supposition that the earth agrees with an ideal black radiator may answer as a first approximation.

In summer, the radiation from a black surface at $+25°$ C. to the sky overhead, if the latter be of a deep blue, may be as if to an efficient radiator of the same quality at a temperature of 0° C.

In winter, under similar circumstances the black surface at $-10°$ C. may radiate to a zenithal sky as if to a screen at $-50°$ C., or $-60°$ C.

A mean of three days of good blue sky in winter and of three more in summer follows:

Winter surface temperature ... $=263°.9$ Abs. C.
Summer surface temperature ... $=291°.3$ Abs. C.
Effective temperature of zenithal sky,
Winter $=212°.2$ Abs. C.
Summer $=269°.9$ Abs. C.

By Stefan's law:

	Winter	Summer
Terrestrial radiation (unabsorbed).	.3819	.5685
Radiation from observed zenithal sky.	.1601	.4196
Difference.	.2218	.1489

Transmission (winter) $T_w = .2218/.3819 = .5806$
Transmission (summer) $T_s = .1489/.5685 = .2619$
Transmission (mean of summer and winter) $= .4213$

We may say that a round 40 per cent. is near enough for an approximate estimate of the average transmission of terrestrial radiation from land surfaces in mid latitudes.

In a note in the *Astrophysical Journal* for September, 1913 (p. 198), Mr. Anders Ångström gives 0.15 gram cal./cm.^{2} min. as an average value of the earth's radiation. This agrees very well with the values which I have obtained in summer, but is smaller than the best winter measures, and to such an extent that one would not suppose that Mr. Ångström had ever observed under conditions most favorable to large transmission. He also declares his "belief that the transmission for clear sky seldom is greater than 25 per cent. and seldom is less than about 5 per cent."[3] The stipulation that the sky must be "clear" rules out those imperfect skies affected by a thin cirro stratus veil, which, as will be evident from my article in the *American Journal of Science,* April, 1913, are included within these limits. My observations, which have been made repeatedly, give a fundamentally different result for the best winter skies.

In desert regions, or for hottest, midday and dry summer conditions, it may sometimes be necessary to increase the estimated surface temperatures considerably, as has been shown above; but this does not apply to more than a small part of the earth's surface, and the principal differences between air and soil temperatures, where the soil is considerably hotter than the air, occur during only a part of

[3] *Op. cit.*, p. 200.

the insolation in the middle of the day, and over water surfaces not at all. Taking the earth as a whole, therefore, the adopted estimate of mean surface temperature, as agreeing with the mean local air temperature, can not be altered more than a fraction of a degree by considering the high temperatures of strongly insolated rock and arid soil, because the area occupied by such surfaces is small compared with the vast expanses of ocean, moist soil, and soil protected by vegetation, which are not thus affected.

Mr. Ångström thinks that, of the terrestrial radiation which escapes absorption by the lower layers of the atmosphere, "a considerable part will be absorbed by the ozone in the higher and colder strata of the atmosphere," and he assigns 20 per cent. *of the total remaining radiation* as a probable value of this absorption. Now if we note that the ozone band covers not one tenth of the entire spectrum, and that 20 per cent. would be a fair value for ozone absorption *within the limits of the band* (at least in summer), we may conclude that the absorption which it exerts is nearer to 2 than to 20 per cent. of the entire spectrum. An example will confirm this approximate statement:

On several occasions of strong ozone absorption, the energy in the solar spectrum of wave-length greater than great ≋ (and for our present purpose it makes little difference whether a curve of solar radiation, or one of terrestrial radiation be taken *in this part of the spectrum*) had a mean value of 537 arbitrary units, as measured on a plotting of the spectral energy-curve. On the same scale, the area covered by the ozone band was equal to 17.4 units, or the ozone absorption was 3.24 per cent. of the spectrum lying beyond the center of the greatest of the bands of aqueous vapor. The following separate values show the variability of the band on days of strongest ozone absorption; $(a) =$ ozone absorption in the band from $9.1\ \mu$ to $10.0\ \mu$ as a percentage of the entire unabsorbed energy between $6\ \mu$ and $20\ \mu$, $(b) =$ ozone absorption of the original unabsorbed energy within the approximate and apparent limits of the band:

Ozone Absorption	(*a*) Per Cent.	(*b*) Per Cent.
Winter measures	3.00	38.8
	3.33	36.9
	3.72	50.0
	3.29	34.5
	3.50	33.3
A single day in July	2.59	21.0
Mean	3.24	35.8

The ozone absorption is considerably smaller in summer than in winter, and ozone probably has its greatest efficiency as a preserver of the earth's heat in the polar regions.

The conditions in my measures of sky radiation were such that the surface temperature could not have differed much from the adopted air temperature, because in winter the sun was low, and in summer the ground was moist; but possibly the values assigned for unobstructed radiation should be lowered to allow for the diminished value of the earth's radiative quality below that of a perfect radiator. This, however, would *increase* the transmission, since the instrument with which the sky radiation was measured was a complete radiator. The thermopile had its very small, blackened, absorbent surface at the center of a hemispherical mirror, 10 cm. in diameter, gold-plated and burnished, the rays entering through a 1 cm. circular, central aperture, entirely open to the outside air. Any rays reflected from the front surface of the thermopile and falling on the mirror, were returned back repeatedly for absorption. In spite of the protection afforded by the case (and by still another, but a wider aperture 2 m. in front of the measuring surface), it was difficult to keep the instrument balanced during very cold or windy weather. Measures were taken in series of five readings. Unless clouds interfered, these readings commonly agreed to the extent indicated in the following examples:

(1) Feb. 2, 1909, $10^{h}20^{m}$ to $10^{h}30^{m}$ A.M. External temperature, $+19.°0$ F. Dew-point, $+13.°5$ F. Relative humidity 76.5 per cent. Wind, fresh W.S.W. Sky, milky blue. A few remnants of dissolving strato cumuli low in the east.

(2) Feb. 3, 1909, $9^{h}30^{m}$ to $9^{h}40^{m}$ A.M. Temperature of snow-covered ground (ther-

mometer bulb ½ cm. in snow) = + 7.°0 F. External air several degrees warmer. In this case the snow temperature is adopted. Relative humidity, 86 per cent. at $8^h\ 58^m$, 74 per cent. at $10^h\ 48^m$. Hoar frost early A.M. Calm. Good blue sky. Cirro stratus bands S.W. Thin mist in valley below.

(3) Feb. 3, 1909. Evening, $8^h\ 25^m$ to $8^h\ 35^m$ P.M. External temperature = + 11.°9 F. Dew-point, + 1.°7 F. Relative humidity, 65 per cent. Little wind. Sky, quite clear.

(1)	Div. −25.8 (2)	Div. −42.5 (3)	Div. −48.8 (4)
Zenithal sky.......	−36.1	−53.0	−35.4
	−28.5	−51.5	−42.3
	−31.9	−48.0	−37.2
	−38.9	−57.1	−44.9
Mean	−32.2	−50.4	−41.7

Small corrections to the galvanometer readings are needed in order to reduce them to a standard time of vibration.

In summer, the successive readings are apt to agree better, not so much on account of any improvement in the sky, but because the thermal and instrumental conditions are more conducive to accuracy. The following are examples for summer:

July 5, 1909	(4) Early A.M. $7^h\ 30^m$ to $7^h\ 40^m$	(5) Noon $11^h\ 50^m$ to 12^h
External temperature	+ 66°.7 F.	+ 75°.0 F.
Dew-point	+ 57.	+ 61.
Relative humidity	72%	62%
	Div.	Div.
	— 32.7	— 30.7
Little wind—Deep blue sky.....	— 33.0	— 29.4
Zenithal sky	— 32.7	— 37.1
Galvanometer deflections	— 32.9	— 27.4
	— 30.9	— 32.0
Mean	— 32.4	— 31.3

The loss of heat by radiation from oceanic areas is much smaller than from land, because of the great absorption of this radiation by the very moist air which constantly hangs over the water. The value of 40 per cent. transmission which I have adopted is for land conditions. In ascribing this value to me without any restriction in his "Note on the Transmission of the Atmosphere for Earth Radiation," Mr. Ångström has overlooked a passage in my paper which I will quote:

> In the very moist tropics, nocturnal cooling is only about half as great [as in temperate regions], while over the ocean the total diurnal change of temperature of the water is less than ½° C.

A computation by Lowell's method gave me for the transmission of terrestrial radiation in the tropics this result; that

> whereas about 60 per cent. of surface heat may be emitted as radiation from temperate regions, only one third as much heat escapes in this way in the tropics [over land surfaces]. It is possible that the low value of 10 per cent. . . . may apply to saturated air over the tropical oceans, where the moisture is in an especially absorbent form.[4]

These illustrations will be sufficient to show the very great variability in the coefficient of transmission of terrestrial radiation.

A further statement in Ångström's note, to the effect that

> only a very weak part of this radiation [namely, from the air at 3,000 m. altitude] reaches the earth's surface,

seems to imply that there is supposed to be some interchange of radiation between bodies of air thus widely separated, although actually an air layer only radiates efficiently from a depth of a few meters. The method employed by Mr. Ångström consists in equating one half of the difference of radiation for black bodies at the temperatures found at top and bottom of an air layer 3,000 m. thick, to the absorption in this layer. The result obtained in this way in the lower air depends entirely upon the thickness assumed for the "effective radiating layer." But this layer, as I have shown elsewhere,[5] can not possibly be 3,000 m. deep, nor is it even 1/100 of that depth, as the investigations of Hutchins and Pearson abundantly prove,[6] and the depth of 3,000 meters

[4] *Astrophysical Journal,* Vol. XXXIV., p. 376, December, 1911.

[5] See "Atmospheric Radiation," Bulletin G, U. S. Weather Bureau, where the efficient radiating layer for carbon dioxide is given as 90 cm., and that for illuminating gas hardly exceeds 20 cm. (*Op. cit.*, p. 62.)

[6] *American Journal of Science* (4), Vol. 18, pp. 277–286, October, 1904. For air "some 60 per cent. of its own radiation is absorbed by a column as thin as 245 cm." (*op. cit.*,

appears to have been chosen to suit the hypothesis.[7] In the isothermal layer, by the same principles, there should be negative absorption! Further comment seems unnecessary. I hasten to add that interchange of radiation between *neighboring* air masses is an essential part of radiant progression through the atmosphere.

General Results

I find that with good blue sky there is radiated from a land surface near the middle of the temperate zone something like a thermal equivalent of 0.15 gram cal./sq. cm. min. in summer, and 0.22 gram cal./sq. cm. min. in winter; and these correspond, respectively, to transmissions of 26 and 58 per cent., the mean transmission being 42 per cent., when the sky is quite clear.

In spite of the higher temperature of land surfaces in summer, there is no greater direct outward radiation from these surfaces than in winter, but even a somewhat smaller one, because the radiation has to pass through a more absorbent atmosphere. When the sky is overcast with clouds, direct surface radiation to space ceases, because the seat of action has simply been transferred to the upper surface of the cloud, where a larger proportion of the incoming solar rays is directly reflected back to space, and that which is absorbed is largely transformed into latent heat to reappear elsewhere after complex atmospheric processes. The whole of the absorbed radiation which is manifested as heat in either earth or air must ultimately return to space as radiation. There are many steps in this process, and the return is retarded in general, though variously retarded, or subject to alternate acceleration and retardation.

There is room here for some obscurity, and perhaps difference of opinion, as to what shall be considered a direct return of terrestrial radiation to space. All of the processes of absorption of solar radiation and its conversion into and emission as terrestrial radiation, involve a thermal mechanism and some delay. If it is insisted that the return must be instantaneous, the only "direct" radiation is that which is reflected back to space. But this is not at all what we mean by terrestrial radiation. If, then, we grant that there must be some delay in the return of radiation to space after transformation into long waves, the return being so speedy that it may fairly be called direct, there is really no reason (since we permit a little delay in order that heat may be conducted between various portions of matter) why we may not include in the direct radiation some of that which is due to heat transferred from the surface to the air by convection, and thence, in turn, radiated from the air by the step-by-step process which alone exists in that medium. In fact, by an observer outside the earth in space, this secondarily radiated heat could not readily be distinguished from that emitted after all only a little more directly. When the problem is attacked by purely meteorological methods, we find that these methods increase the computed nocturnal radiation, making it as high as 58.5 per cent. of the total loss of surface heat by radiation and convection combined, according to the method developed by Dr. Percival Lowell in his "Temperature of Mars."[8]

Lowell's equation for nocturnal cooling may be put in the form

$$\frac{T - \Delta T_1}{T - \Delta T_2} = \sqrt[4]{\frac{y(1 - ae)}{y(1 - be)}},$$

where T is the average day temperature on the absolute scale, ΔT_1 is the nocturnal cooling with clear sky, ΔT_2 is the same with cloudy

p. 283). The agreement of the air transmission of 40 per cent. found by these investigators in this laboratory experiment with the value which I have given for the entire atmosphere may be only a coincidence; but the fact remains that there are extensive regions of the spectrum which are not emitted freely by air, and which therefore are not much absorbed by the atmosphere and do not take part in its interchange of radiation.

[7] Certain layers in the atmosphere, which are cloud-laden and at various heights, stop all outgoing radiation and appear as if at the same temperature as the surface, even though they may be 20°, or more, colder, and the difference of temperature bears no relation to the absorption by the intervening air.

[8] *Proc. American Academy of Arts and Sciences*, Vol. XLII., No. 25, March, 1907, p. 660–661.

sky, y is the radiant energy received at the earth's surface, e is the "relative emissivity," or rather, it is the *effective* emissivity from the surface through the air, that is, the radiant emission as affected by atmospheric absorption of rays of long wave-length, and which is governed by the fourth-power Stefan law, but it is distinguished from the convective loss, except in-so-far as these two overlap according to the principle under discussion. The coefficients a and b are numbers derived from the observed transmission of instrumental radiation to the sky applied to the meteorological data under clear, and cloudy, nocturnal conditions, respectively.

The equation rests upon the observed fact that $\Delta T_1 : \Delta T_2 = 5:2$, which holds good for both tropical and temperate regions; also upon the fact that there is no difference between the mean temperature of clear and cloudy days; and the numerical coefficients are chosen on the assumption that the average air transmission is the same for either day or night. This I find to be justified so far as the radiation of the measuring instrument to the sky is concerned, but it does not hold, in general, for the surface of the ground. Hence there arises a discrepancy. For example, if we let $T =$ the mean day temperature, 292° Abs. C., and take the average transmission of instrumental radiation as 40 per cent. for clear sky, we have the effective transmission of a unit of energy with a cloudy sky $= 2/5 \times 0.40 = 0.16$, and the average transmission for day sky, assuming that there are as many clear days as cloudy, is 0.28. The mean transmission for an average day and a clear night becomes

$$a = \tfrac{1}{2}(0.28 + 0.40) = 0.34;$$

and the mean effective transmission for an average day and a cloudy night is

$$b = \tfrac{1}{2}(0.28 + 0.16) = 0.22.$$

For mean temperate values,

$$\frac{T - \Delta T_1}{T - \Delta T_2} = \frac{292 - 10}{292 - 4} = .9792,$$

$$\frac{1 - .34e}{1 - .22e} = (.9792)^4 = .9196,$$

$$e = .585.$$

But this is practically the effective transmission of the earth's radiation, because the emissivity of the earth is nearly that of a black body. Nevertheless, e by the computation is nearly 50 per cent. larger than the transmission of instrumental radiation with which we started, so that, in round numbers, the equation as it stands raises the transmission from .40 to .60, mean $=$ .50, which is the value adopted by Lowell. The explanation of the discrepancy between the transmission obtained from measures of sky radiation, and that from nocturnal cooling, appears to lie in the reconversion of a part of the heat abstracted from the surface by convection currents, into radiation which is added to the radiation emitted as such from the ground; but the large part of the energy communicated to the air by convection remains in the circulation of the air so long that it does not affect the diurnal changes on which the equation is based.

A similar computation for the tropics gave the result that e has only about one third of the value derived from measures in the temperate zone, while the discrepancy is very much smaller and has the opposite sign. This may mean that the excessive evaporation and precipitation of the tropics bring thermal losses and gains which still further complicate the relations between radiation and convection. Evidently there may be some ambiguity about the term "terrestrial radiation," unless we limit our definitions very carefully.

As a check upon these measures I have used my determination of the transmission of the proper lunar radiation by the earth's atmosphere in which values as high as 48 per cent. were obtained in winter. The transmission of lunar radiation is relatively smaller than that for terrestrial radiation from a land surface by the same atmosphere, because, owing to the higher temperature of the lunar surface, its radiation invades regions of the spectrum where the atmospheric absorption is especially large.[9]

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December, 1913

[9] See F. W. Very, "Sky Radiation and the Isothermal Layer," *American Journal of Science*, Vol. XXXV., p. 379, April, 1913.